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VI.—NUMERICAL VALUES OF CHEMICAL CONSTANTS AND FREQUENCIES OF THE ELEMENTS.

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Received August 7, 1924.

ABSTRACT.

(1) The experimental evidence for the general chemical constant C_0 possessing the theoretical value (—1·589) is collected.

(2) Tables of chemical constants are given and data collected for the determination of characteristic temperatures of the elements.

(3) A linear relation between logarithm of mass and characteristic temperature is indicated.

INTRODUCTION.

IT has been pointed out (Egerton, Phil. Mag., 39, p. 1, 1920) that the value of Stephan's constant can be determined from the value of the chemical constant obtained from vapour pressure measurements, and that the value so obtained in the case of mercury did not agree satisfactorily with that obtained from the later measurements of total radiation. The object of this communication is to modify that statement in accordance with recent work on vapour pressures and to show that there is no longer a general discrepancy. Accepting, then, the latest figures for radiation constants, the values of the chemical constants of the elements are computed. Values of vibration frequency are also calculated for the same elements. The chemical constant depends on the logarithm of atomic weight, and it is noticed that the frequencies of some of the closely related elements can be expressed as a linear function of log M.

NUMERICAL VALUES OF CHEMICAL CONSTANTS.

The chemical constant or the constant of the vapour pressure equation for a solid element with monatomic vapour, viz.,

$$\log p = -\frac{\lambda_0}{RT} + \frac{C^g_p}{R} \log T - \int_0^T \frac{dT}{RT^2} \int_0^T C^s_p \, dT + \text{constant}$$

is given by the Sackur relation $C = 3/2 \log M - C_0$ where

$$C_0 = \log \frac{2\pi^{3/2} R^{5/2}}{N^4 h^3}$$

(M is the atomic weight, N the Avrogadro number, h Planck's constant). C_0 becomes $-1{\cdot}589$, if $h = 6{\cdot}554 \cdot 10^{-27}$, $N = 6{\cdot}061 \cdot 10^{23}$, $R = 8{\cdot}315 \cdot 10^7$ or ($k = 1{\cdot}372 \cdot 10^{-16}$).

The figure implies, or alternatively can be based upon, values of the Stephan constant $5{\cdot}709 \cdot 10^{-5}$, or Wien's constant 0·2885, which are in close agreement with the most recent experimental values* (c.f. Birge, Phys. Rev., 14 . 361, 1919; and Nature, June 16, p. 811, 1923).

* NOTE.—These are the values accepted for the compilation of Critical Tables of Constants and are in close accord with those cited in Landolt Bornstein (1923 edition).

Experiment justifies the application of the Sackur relation; the evidence is collected in the following table. It depends entirely on vapour pressure and specific heat measurements, except in the cases of bromine and chlorine :—

TABLE I.

Element.	C Expl.	C_0.	Reference.
Mercury	$1{\cdot}83 \pm 0{\cdot}03$	$1{\cdot}62 \pm 0{\cdot}03$	Nernst
Cadmium	$1{\cdot}45 \pm 0{\cdot}1$	$1{\cdot}63 \pm 0{\cdot}10$	Egerton. Braune
Zinc	$1{\cdot}10 \pm 0{\cdot}1$	$1{\cdot}62 \pm 0{\cdot}10$	,, ,,
Lead	$1{\cdot}8 \pm 0{\cdot}2$	$1{\cdot}67 \pm 0{\cdot}20$	Egerton
Chlorine	$0{\cdot}80 \pm 0{\cdot}1$	$1{\cdot}52 \pm 0{\cdot}1$	Henglein
Bromine	$1{\cdot}27 \pm 0{\cdot}1$	$1{\cdot}58 \pm 0.1$	,,
Argon	$0{\cdot}79 \pm 0{\cdot}04$	$1{\cdot}61 \pm 0{\cdot}04$	Born
Hydrogen	$-1{\cdot}11 \pm 0{\cdot}03$	$1{\cdot}57 \pm 0{\cdot}03$	Simon
Cæsium	$1{\cdot}64 \pm 0{\cdot}16$	$1{\cdot}54 \pm 0{\cdot}16$	Scott
Rubidium	$1{\cdot}36 \pm 0{\cdot}18$	$1{\cdot}53 \pm 0{\cdot}18$	,,

The weighted mean would become $1{\cdot}596 \pm 0{\cdot}008$, a value very near that deduced above from the accepted basic constants. The small discrepancy pointed out previously (Phil. Mag., 39, p. 1, 1920) seems to exist no longer.*

NUMERICAL VALUE OF FREQUENCIES.

The tables which follow include the chemical constants for the monatomic vapour of the elements based on the value $C_0 = -1{\cdot}589$. These tables also include the characteristic temperatures ($\beta\nu$) computed according to the Lindemann melting point formula (Column 7) and according to a formula based on the coefficient of expansion (Column 8) from the data given in 3, 4 and 5, collected from Landolt Bornstein (1923 edition) and other sources. Doubtful values are placed in brackets. $\beta = h/k = 4{\cdot}778.10^{-11}$.

Previous computations of the values of $\beta\nu$ have been made by Lindemann and subsequently by Biltz (Zeit. Elektrochem, 17 676, 1911) on the basis of Lindemann's formula, $\nu = K\sqrt{\frac{T_s}{MV^{2/3}}}$. The constant K also used by Biltz had the value $2{\cdot}12 \times 10^{12}$, instead of $3{\cdot}1 \times 10^{12}$, used above. Blum (Ann. d. Phys., (4) 42, 1,397, 1913) has compared the merits of the formulæ of Lindemann, Einstein, Debye, Gruneisen and others. Since $T_s a$ is approximately constant (as pointed out by Lindemann), the formula $\nu = 4{\cdot}2{\cdot}10^{11}\sqrt{\frac{1}{Mav^{2/3}}}$ also applies, T_s is the melting point and a the coefficient of expansion; Blum showed that it agreed well with experimental results. This formula is related also to that of Grüneisen when $C_v = 3R$. None of the formulæ take into account the anisotropic nature of the solid elements. On the whole, the Lindemann melting point formula agrees closest with experimental results. The figures in brackets in the last column have been calculated from the available specific heat data, but only in some cases are the data reliable. The values not in brackets are generally accepted as correct. Although the measurements may be satisfactory, the state of the metal

* This point is being subjected to further scrutiny in the cases of mercury and zinc.

may cause considerable difference in the specific heats, e.g., for calcium two forms have been noted by Eastman, Williams and Young (J.A.C.S., 46, 1178, 1924), giving differences in specific heat, likewise in the case of tin (*see* Brönsted, Zeit. Phys. Chem., 88, 479, 1924). The values for iron also differ widely. In some cases (e.g., sulphur) a single maximum vibration frequency is insufficient to account for the variation of specific heat, as might be expected in accordance with Born and Kármán's theory, which takes into account the structure of the crystal unit.

That there is a relation between frequency and mass in accordance with the periodic law is well known; mass enters into all the formulæ for the frequency since they are all based on the simple pendulum formula $\nu=\frac{1}{2\pi}\sqrt{\frac{F}{M}}$ but none of the theoretical formulæ are completely supported by the results of specific heat determinations. Only at very low temperatures should one expect the characteristic frequencies calculated from the elastic constants to agree perfectly with those calculated from specific heat data. Accurate data for the elastic constants at such temperatures are not, however, available (*vide* Jeans "Report on Radiation and Quantum Theory," 2nd edition, 1924).

If $\beta\nu$ is plotted against $\log M$ (*see* diagram), similar elements tend to fall on straight lines to a common point, whereabouts Hg, Pb, Bi and Tl are situated (elements with high atomic weight and low melting point). A relationship of this sort implies a rather complicated connexion between melting point and mass, for if $\log M=a+b\nu$ where a and b are constants, and $\nu=K\sqrt{\frac{T_s}{MV^{2/3}}}$,

$$T_s=\frac{1}{b^2K}\cdot MV^{2/3}\cdot(a^2-2a\log M+\log M^2).$$

It is noteworthy, however, that agreement appears to be closer for the most reliable values of $\beta\nu$ obtained from specific heat data than for those calculated from the melting-point formula (e.g., Cd, Zn and Ag, Cu).

TABLE I.—*Group* 1*a*.

	Chem. Const.	Atomic volume.	M. Pt. °C.	$\alpha \times 10^{-5}$	$Cp-Cv$† 20°C.	$\beta\nu$ 1	$\beta\nu$ 2	$\beta\nu$ obs.
Li	—0·327	13·0	179	6·0	0·33	508	418	(330) Dewar, (450) Koref, Laemmel, &c.
Na	0·453	23·8	97·9	7·2	0·5	207	171	125 Günther, (160) E. and R.,* Dewar, 170 Griffiths, Koref, &c.
K	0·798	45·5	63·5	8·3	0·52	122	99	(70) E. and R., (100) Dewar.
Rb	1·309	56·2	39·0	9·0	0·60	74	60	...
Cs	1·596	71·0	28·5	9·7	0·68	54	43	...

* Eastman and Rodebush.

† *See also* Eastman, Williams and Young (loc. cit.).

The specific heat measurements vary too much amongst themselves to show any conclusions as to the relationships between the values of the frequency for this group of elements (*see*, however, the diagram).

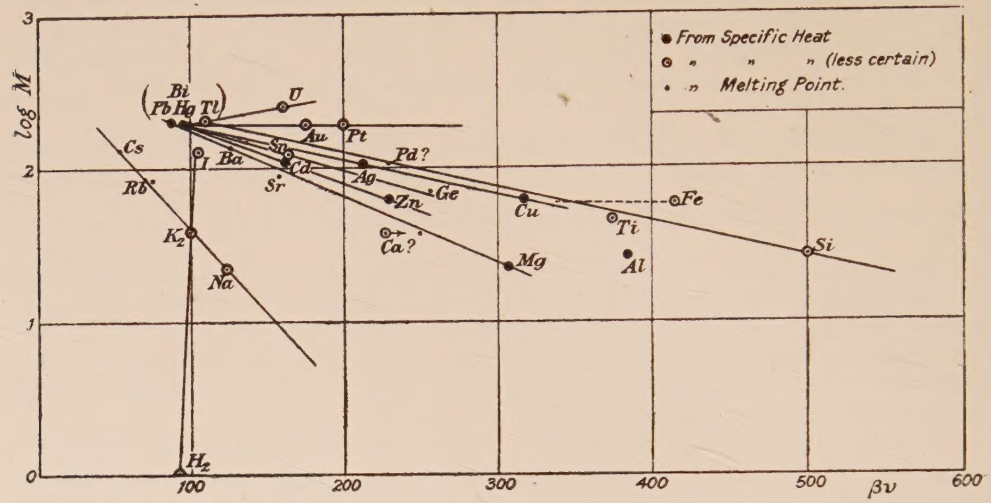


TABLE II.—*Group* 1*b*.

	Chem. const.	Atomic volume.	M. Pt. °C.	$\alpha \times 10^{-5}$	Cp—Cv 20°C.	$\beta\nu$ 1	$\beta\nu$ 2	$\beta\nu$ obs.
Cu	1·116	7·12	1,083	1·628	0·166	356	324	315 Keesom and Onnes, Nernst, Griffiths, &c.
Ag	1·460	10·3	960·5	0·763	0·24	248	223	215 Nernst, Koref, Griffiths, &c.
Au	1·853	10·2	1,063	1·36	0·20	178	177	(175) Dewar, Richards and Jackson.

The results for silver (215) and copper (315) are known with considerable certainty. A line drawn through these points passes through the mercury point. The value for gold (175) would need to be halved to fall on this line.

TABLE III.—*Group* 2*a*.

	Chem. const.	Atomic volume.	M. Pt. °C.	$\alpha \times 10^{-5}$	Cp—Cv 20°C.	$\beta\nu$ 1	$\beta\nu$ 2	$\beta\nu$ obs.
Be	0·156	4·9	1,278	...	...	1,143	...	(800) Dewar, (960) Humpidge, Nilson, &c.
Mg	0·490	14·0	650	2·47	0·20	379	318	(305) Nernst and Schwers, Eastman and Rodebush.
Ca	0·815	25·8	804	1·68	0·08	260	...	226 Günther.
Sr	1·325	34·5	(800)	...	...	159	...	...
Ba	1·618	38·2	(850)	...	...	125	...	(100) Dewar.

The same remarks apply as for Group 1a. The value of $\beta\nu$ for magnesium 305 appears to be fairly well fixed by the measurements of Nernst and Schwers, Eastman and Rodebush, and others, but the value for calcium is open to doubt. Rodebush has shown that two forms exist with a transition point about 400°C. It is likely that the value $\beta\nu=226$ is a mean of a lower value and a higher value (about 250).

TABLE IV.—*Group 2b.*

	Chem. const.	Atomic volume.	M. Pt. °C.	α $\times 10^{-5}$	$Cp—Cv$ 20°C.	$\beta\nu$ 1	$\beta\nu$ 2	$\beta\nu$ obs.
Zinc	1·134	9·2	419·4	2·83	0·28	230	222	228 Nernst.
Cadmium	1·487	13·0	320·9	2·88	0·25	151	150	168 Rodebush, Griffiths, Ewald, Behn, Barschall.
Mercury	1·864	14·2	—38·89	2·96	...	66	107	96 Simon.

The values of $\beta\nu$ obtained from the specific heat measurements for these three metals fall close to a straight line. (The value for cadmium is perhaps a little too large; the recent measurements of Rodebush taken alone lead to a value $\beta\nu=163$).

TABLE V.—*Group 3a* .

	Chem. const.	Atomic volume.	M. Pt. °C.	α $\times 10^{-5}$	$Cp—Cv$ 20°C.	$\beta\nu$ 1	$\beta\nu$ 2	$\beta\nu$ obs.
B	—0·038	6·3	2,200	...	...	1,505	...	(1,130) Koref.
Al	0·56	10·0	660·0	2·184	0·23	404	384	(386) Nernst and Schwers and others.

The values for scandium and the rare earth metals are not known, so that no relationship can be established between the frequencies in this group. The value for Boron is doubtful; Dewar's measurement indicates a lower figure.

TABLE VI.—*Group 3b.*

	Chem. const.	Atomic volume.	M. Pt. °C.	α $\times 10^{-5}$	$Cp—Cv$ 20°C.	$\beta\nu$ 1	$\beta\nu$ 2	$\beta\nu$ obs.
Ga	1·176	11·8	29·75	...	...	136	...	...
In	1·501	15·8	154	4·0	...	114	118	...
Tl	1·877	17·2	301·5	3·0	0·30	96	99	(99) Nernst and Schwers (Ewald, &c.)

In this group thallium is the only element for which the specific heats have been accurately determined. The values obtained from the melting point formula for this group appear to agree well with those observed (e.g., Al and Tl). The $\beta\nu$ values for Ga, In, Tl lie close about a straight line.

TABLE VII.—*Group 4a.*

	Chem. const.	Atomic volume.	M. Pt. °C.	$\alpha \times 10^{-5}$	$Cp-Cv$ 20°C.	$\beta\nu$ 1	$\beta\nu$ 2	$\beta\nu$ obs.
C	0·030	3·42	...	...	...	...	...	1,850 Nernst.
Si	0·583	12·0	1,413	...	...	502	...	(500) Nernst and Schwers.
Ti	0·931	10·6	(1,800)	...	...	444	...	(375) Dewar.
Zr	1·350	...	(1,530)	...	...	355	...	(225) Dewar.
Hf	...	...	...	...	...	...	...	...
Th	1·960	...	(1,690)	...	...	156	...	(110) Dewar.

In this group the values of $\beta\nu$ from the specific heat data are unreliable, except in the case of diamond and silicon. The value for silicon obtained from measurements above 100°C. is higher than the value given above (Richards and Jackson's and Russell's results give values over 600 ; Russell giving different values for two forms of silicon). The value of $\beta\nu$ depends on the state of the element.

TABLE VIII.—*Group 4b.*

	Chem. const.	Atomic volume.	M. Pt. °C.	$\alpha \times 10^{-5}$	$Cp-Cv$ 20°C.	$\beta\nu$ 1	$\beta\nu$ 2	$\beta\nu$ obs.
Ge	1·200	13·5	958·5	...	...	258	...	(280 ?) (Nilson Petterson).
Sn	1·575	16·3	231·8	2·14	0·25	120	157	(166) Rodebush, 198 (white) 253 (grey) Brönsted.
Pb	1·886	18·3	326·9	2·76	0·43	96	101	88 Nernst, Onnes and others.

The specific heat of germanium not being known accurately it is not possible to draw any conclusions from this set of elements. Another difficulty is the allotropy of tin. Bronsted gives the values 198 for white and 253 for grey tin, but more recent specific heat measurements indicate a lower value for white tin. A line passing through lead and tin (166) passes near the point 258, calculated from the melting point of germanium.

TABLE IX.—*Group 5.*

	Chem. const.	Atomic volume.	M. Pt. °C.	$\alpha \times 10^{-5}$	$Cp-Cv$ 20°C.	$\beta\nu$ 1	$\beta\nu$ 2	$\beta\nu$ obs.
P	(0·749)	17·0	44·1	...	...	184	...	420 Ewald.
As	(1·223)	13·1	...	...	...	232	221	(295) Ewald.
Sb	(1·539)	18·3	630·5	0·976	0·05	15·3	...	(140) Günther.
Bi	(1·891)	21·4	271	1·26	0·09	98	(161)	(100 ?) Dewar, Richards, and Jackson.

The values for these elements are also doubtful. Ewald's value does not quite agree with Dewar's for white phosphorus or for bismuth. Gunther's value for antimony is very considerably less than those obtained from the work of Dewar, Ewald, &c., which indicate a value obout 190. The value for bismuth is also uncertain.

TABLE X.

	Chem. const.	Atomic volume.	M. Pt. °C.	α $\times 10^{-5}$	$Cp—Cv$ 20°C.	$\beta\nu$ 1	$\beta\nu$ 2	$\beta\nu$ obs.
V	0·972	9·1	(1,720)	...	...	444	...	...
Nb	1·364	7·35	(1,950)	...	...	372	...	...
Ta	1·799	10·9	(2,800)	0·65	...	275	263	(270) Siemens and others.

The frequency determined from the melting point gives approximately a straight line for these elements, but there exist no satisfactory specific heat data.

TABLE XI.—*Group* 6.

	Chem. const.	Atomic volume.	M. Pt. °C.	α $\times 10^{-5}$	$Cp—Cv$ 20°C.	$\beta\nu$ 1	$\beta\nu$ 2	$\beta\nu$ obs
S	(0·670)	15·5	(112·8)	7·0	...	206	170	(280) Dewar. (170-430) Nernst, Koref.
Se	(1·259)	16·5	220	...	...	145	126	(190) Dewar.
Te	(1·569)	20·6	(453)	...	...	129	...	(120) Tilden, (150) Dewar.

For sulphur Nernst gave $Cv=\frac{1}{4}F(74)+\frac{3}{4}F(510)$; it is probable the specific heats of selenium and tellurium are also dependent on more than a single frequency maximum.

TABLE XII.

	Chem. const.	Atomic volume.	M. Pt. °C.	α $\times 10^{-5}$	$Cp—Cv$ 20°C.	$\beta\nu$ 1	$\beta\nu$ 2	$\beta\nu$ obs.
Cr	0·935	7·76	(1,550)	0·824	0·036	443	490	(460) Dewar, Richards and Jackson.
Mo	1·384	9·4	(2,500)	0·506	0·03	377	434	(300) Dewar, ? Richards and Jackson.
W	1·808	9·65	(3,370)	0·445	0·05	322	329	(275) Dewar.
Ur	1·976	12·7	(1,350)	...	...	166	...	(160) Dewar.

For this set of elements the values determined from the specific heat data are only approximate values, but they agree fairly well with the melting point formula,

except in the case of molybdenum. The values of $\beta\nu$ from the melting point formula for Cr, Mo, W fall on a straight line (uranium is not on this line).

TABLE XIII.—*Group* 7.

	Chem. const.	Atomic volume.	M.Pt. °C.	α $\times 10^{-5}$.	$Cp-Cv$ 2 °C.	$\beta\nu$ 1	$\beta\nu$ 2	$\beta\nu$ obs.
H_2	—1·584	(13·1)	—259	...	...	...	...	91 Simon.
F	0·329	(12·4) ?	—223	...	...	78	...	...
Cl	0·736	(16·2) ?	—101	...	...	129	...	...
Br	1·265	(19·2) ?	—7·2	...	...	100	...	...
I	1·566	25·7	113	(8·4)	...	87	66	106 Nernst and Schwers.

It is possible that the values of $\beta\nu$ for these elements are nearly the same (*see* diagram).

	Chem. const.	Atomic volume.	M. Pt. °C.	α $\times 10^{-5}$.	$Cp-Cv$ 20°C.	$\beta\nu$ 1	$\beta\nu$ 2	$\beta\nu$ obs.
Mn	1·021	7·54	(1,264)	2·22	0·35	399	295	(320).

(Dewar's results give $\beta\nu=340$, but results of Richards and others indicate a value about 300.)

TABLE XIV.—*Group* 8.

	Chem. const.	Atomic volume.	M.Pt. °C.	α $\times 10^{-5}$.	$Cp-Cv$ 20°C.	$\beta\nu$ 1	$\beta\nu$ 2	$\beta\nu$ obs.	Dewar.
Iron ...	1·031	7·11	1,552	1·1	0·1	438	417	(415) Ewald, Günther, Richards, Behn.	(380)
Cobalt ...	1·067	6·70	1,490	1·234	0·13	429	394	(360) Richards, Tilden	(345)
Nickel ...	1·064	6·68	1,452	1·23	0·16	426	393	(345) Richards, Schimpf, Tilden, Behn.	(345)
Ruthenium	1·422	8·3	(1,950)	0·96	...	342	317	...	(360)
Rhodium	1·429	8·5	1·970	0·85	...	269	264	...	(320)
Palladium	1·453	9·3	1,555	1·17	0·21	292	270	(230) Richards, Behn.	(255)
Osmium	1·832	8·5	(2,500)	0·65	...	277	279	...	(235)
Iridium ...	1·840	8·63	2,355	0·649	0·076	266	266	(240) Behn.	(265)
Platinum...	1·847	9·13	1,755	0·893	0·117	229	230	(200) Richards, Behn Tilden.	(210)

Even in the case of iron the specific heat measurements lead to values of $\beta\nu$ varying from 360-460. The latest measurements of Günther at low temperatures

give a value about 453. Nothing very definite is available as to the frequencies of these elements.

The lighter elements Li, Be, B, C appear to have frequencies slightly more than double those which would be necessary for them to lie on the lines on which the rest of their group resides.

The evidence is insufficient to draw any definite conclusion yet as to the relationships between the characteristic temperatures of the elements, and in many cases a single frequency is not applicable. All that can be said is that in certain cases $\beta\nu$ can be expressed as a straight line function of the logarithm of mass, and that there is a general tendency for a function of this sort to apply.

DISCUSSION.

The PRESIDENT said that the Author had collected a mass of evidence linking up experimental results with theoretical formulæ in a very interesting way. His Paper dealt with subject matter of a kind not often brought before the Society, and would doubtless stimulate others to undertake research on the numerous unsettled questions he had touched upon.

Prof. H. S. ALLEN (communicated): As I have pointed out in a Paper on "Atomic Frequency" (Phil. Mag., Vol. 34, page 478, 1917), later writers seem to have overlooked the important work of Sutherland on the subject of atomic vibrations in solids. As long ago as 1890 he claimed to have found simple relations between the *periods* of vibration of elements at the melting-point for several chemical families. It is a characteristic feature of Sutherland's theory of fusion that melting occurs when the space between the molecules attains a certain value relatively to the size of the molecules. Thus $\alpha\ T_S$ is constant, a relation afterwards verified by Gruneisen. Sutherland's theory leads directly to Lindemann's formula for the atomic frequency at the melting-point.

With our present knowledge of the structure of crystalline solids it seems essential that in future investigations of atomic frequencies the arrangement of the atoms in a space lattice should be considered. The lattice theory of atomic heats due to Born and Kármán certainly presents great difficulties as regards calculation, but it must be a much closer representation of the actual conditions than the earlier theories.

VII.—THE SENSIBILITY OF CIRCULAR DIAPHRAGMS FOR THE RECEPTION OF SOUNDS IN WATER.

By J. H. POWELL, *M.Sc.*, *F.Inst.P.*

Received September 14, 1924.

ABSTRACT.

The Paper describes an investigation of the sensibility of circular diaphragms as receivers of submarine sounds. The diaphragms were mounted with one face immersed in water and to the other was attached a microphonic or electro-magnetic detector. The diaphragms were all designed to have a frequency of 850 ∽ under these conditions.

The response of the diaphragms to sounds of a single definite frequency was measured and resonance curves were obtained from which the magnitude of the damping due to the detector was determined. A value was also obtained for the concentration of energy in the restricted volume of the experimental tank, and an investigation was made of the effect of the proximity of sound reflectors and of the vibrating diaphragm of the source of sound.

It was shown that when corrections were made for these various effects, the values for the resonance amplitude and for the persistence of vibration were consistent with those determined mathematically by Prof. H. Lamb for diaphragms under ideal conditions.

The investigation was extended to cover complex sounds or "noises" of no definite pitch, and the results were again consistent with theoretical anticipations.

IN a previous communication* an account is given of an investigation of the natural frequency of circular diaphragms when vibrating under different conditions in air and water. The object of the present research has been to extend this investigation to the sensibility of these diaphragms when used to detect sounds in water, and to determine the law governing the amplitude of vibration of diaphragms when actuated by sound waves in water.

The mathematical treatment of the subject is again due to Prof. H. Lamb, and may be summarised briefly as follows :—

The displacement x of the centre of a circular diaphragm, radius a, vibrating with one side in contact with water, is subject to an equation of the form

$$A\ddot{x}+B\dot{x}+Cx=P\cos pt \qquad (1)$$

where A is a factor equal to the effective mass of the diaphragm, including the additional inertia due to the movement of the water in the vicinity of the diaphragm. B is a factor representing the damping of the diaphragm which is supposed to result entirely from the radiation of energy to the water in the form of sound waves. C represents the restoring force acting on the diaphragm corresponding to unit displacement of the diaphragm from its equilibrium position.

The numerical values of these factors can be deduced from equations (5), (26) and (4) respectively in Prof. Lamb's Paper on "The Vibration of an Elastic Plate in Contact with Water."†

* Proc. Phys. Soc., Vol. 35, April (1923), "On the Frequency of Vibration of Circular Diaphragms."

† Proc. Roy. Soc., A, Vol. 98, p. 205 (1920).

P is the value of the total pressure due to the sound waves on the diaphragm, and $n=\frac{p}{2\pi}$ is the applied frequency.

This gives for the maximum displacement of the centre of the diaphragm

$$D=\frac{P}{\sqrt{(C-p^2A)^2+p^2B^2}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

In the case of resonance, the natural frequency of the diaphragm n_0 is equal to that of the sound waves acting upon it.* Then

$$p_0=2\pi n_0=\sqrt{\frac{C}{A}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

$$D_{\text{max.}} \text{ being equal to } \frac{P}{p_0 B.} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

If now p_0 is regarded as a constant, B varies as a^4 and P may be assumed proportional to the area of the diaphragm, i.e., varying as a^2.

$$D_{\text{max.}} \propto \frac{a^2}{a^4}, \text{ i.e., as } \frac{1}{a^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Therefore, for diaphragms having the same frequency in water, the amplitude at resonance should be inversely proportional to the square of the radius.

The value of $D_{\text{max.}}$ is also independent of A. Therefore, the mass of the diaphragm has no influence on the maximum amplitude at resonance. If a diaphragm is loaded by a mass M at its centre, the quantity A becomes $A+M$, and D is correspondingly less at frequencies away from the resonance point. Fig. 1 is a theoretical amplitude-frequency curve calculated for diaphragms 3 in. in diameter, frequency 850 ~ in water, with and without a 20-gramme load at the centre. The resonance curve is much sharper in the case of the loaded diaphragm, but the maximum amplitude remains the same.

The amplitude of a damped vibrating diaphragm diminishes according to the law $e^{-\alpha t}$,

$$\text{where } \alpha=\frac{5}{36}\;\frac{\rho}{\rho_1}\;\frac{p^2a^2}{(1+\beta)\,hc} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \dagger(6)$$

The quantity α is numerically equal to the ratio of the damping factor to the mass factor, i.e., to the quantity B/A.

When a diaphragm is responding to a source of sound of variable frequency, the energy of resonance falls to one half its maximum value when the frequency of the sounder differs from that of the diaphragm by the fraction α/p, i.e., if the amplitude at resonance is reduced to half value by reducing the frequency z vibrations, then

$$\alpha=\frac{B}{A}=2\pi z \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

where the value of B is that for resonance. This resonance value of B is always assumed throughout the remainder of this Paper.

* The damping, though considerable, is not sufficient to affect the frequency appreciably. Lamb, loc. cit.

† Lamb, *ibid.*, equation (29).

The object of this research was therefore to confirm these theoretical deductions and to examine the shape of the experimental resonance curves in order to determine from them the most sensitive type of diaphragm for the reception both of pure notes and for general sounds. In most practical cases a detector, usually of the microphone type, must be attached to the diaphragm, and this will modify both A and B. Experiments were therefore made to determine the loading and damping effects of detectors on the sensitiveness of a diaphragm.

All the experiments were carried out in a tank 12 ft. long, 3 ft. wide, and $2\frac{1}{2}$ ft. deep, the sides of which being of brick, coated with a smooth surface of tar, acted as sound reflectors. The assumption that the energy is dissipated from a vibrating diaphragm in the form of semicircular waves was therefore not applicable in the case, and in the following experiments the effect of this concentration of energy on the value of B was actually measured.

An investigation was also made of the results produced by various local con-

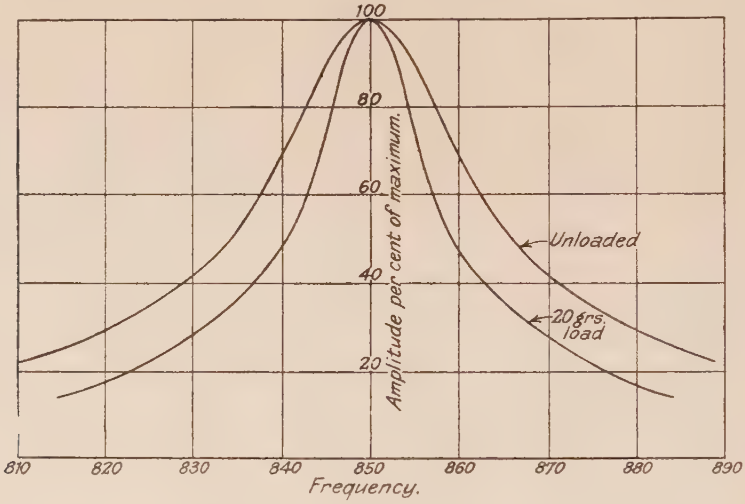


Fig. 1.—Theoretical Resonance Curve for 3 in. Steel Diaphragms (Loaded and Unloaded, Frequency 850 ∿ in Water). No Additional Damping.

ditions on the value of B, including the effects of the proximity of the wall of the tank, and of the vibrating diaphragm of the sounder.

Experimental Methods.

The diaphragms which were used in connexion with this investigation were all of the same type as those used in the earlier investigations of the frequency, and a series was constructed all having the same fundamental frequency when vibrating with one side in contact with water. To avoid accidental effects all the experiments were repeated with a duplicate set of diaphragms. The frequency selected was 900 vibrations per second, and the actual frequencies of the diaphragms usually lay well within 5 per cent. when loaded with their respective detectors. This load (kept constant during the experiment) caused a slight lowering of the fundamental frequency, which was now about 850 ∿ .

The general description of the diaphragms and the method of supporting them in their massive steel holder has already been described in the Paper referred to previously.

The sounds employed to excite the diaphragms were of two main classes—(*a*) a pure note of a frequency which could be varied continuously from 100 to 2,000 or more vibrations per second, and which was used more particularly for the investigation of the diaphragms in their resonance region; and (*b*) complex sounds, the constituents of which were in general different from the fundamental frequency of the diaphragm. For the investigation of the response of the diaphragms to complex sounds, a source emitting simultaneously sounds of all frequencies from 0 to 2,000 ∿, and of equal intensity, would have been most desirable, but in its absence, different sources giving out a certain amount of general noise were employed for this purpose. The sources of complex sound which were principally used were as follows:—A clock lying at the bottom of a deep vessel floating in the water; a motor running on the wall of the tank with an attachment to throw it out of balance and to produce a loud rattling sound; an electric fan on the wall of the tank; a pendulum which

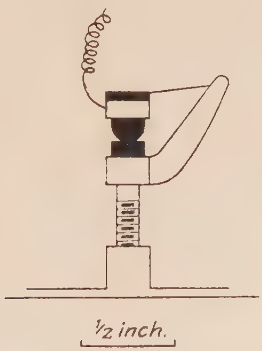


FIG. 2.—SIMPLE CARBON CONTACT MICROPHONE.

was allowed to fall from a fixed height and strike the wall of the tank; and a stream of air bubbles delivered from a tube immersed in the water of the tank.

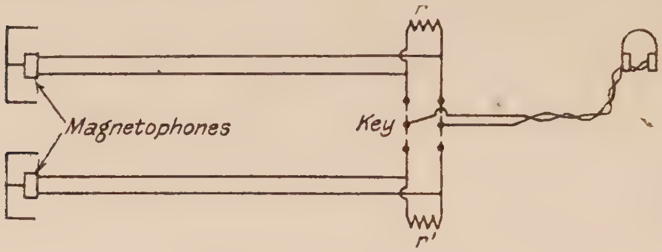
The resultant vibrations of the diaphragms were detected by the following means:—(*a*) A "Brown" telephone (or magnetophone), the reed of which was attached to the centre of the diaphragm; (*b*) a granular microphone of the standard type made by the Automatic Telephone Manufacturing Co.; (*c*) a small form of simple carbon contact microphone (S.C.C.)—Fig. 2—constructed specially for these experiments.

METHOD OF COMPARING DIAPHRAGMS.

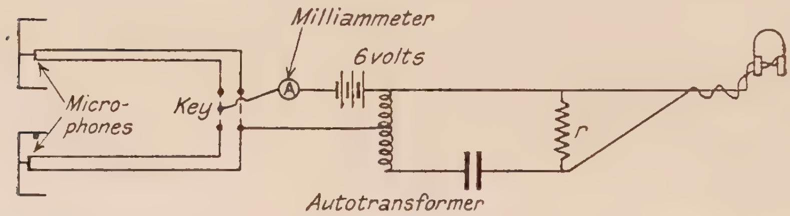
Fig. 3, (*a*) and (*b*), shows the method of comparing the diaphragms. The two diaphragms were suspended in the water equidistant from the source of sound, and the detectors connected in turn to the listening telephone. A shunt was introduced into the circuit of the more sensitive diaphragm and was adjusted until the sounds heard were equal or, alternatively, similar shunts were introduced into both circuits and adjusted to extinguish the sound heard altogether.

From the values of these shunts the ratio of the loudness of the sound heard in the two cases is readily calculable.*

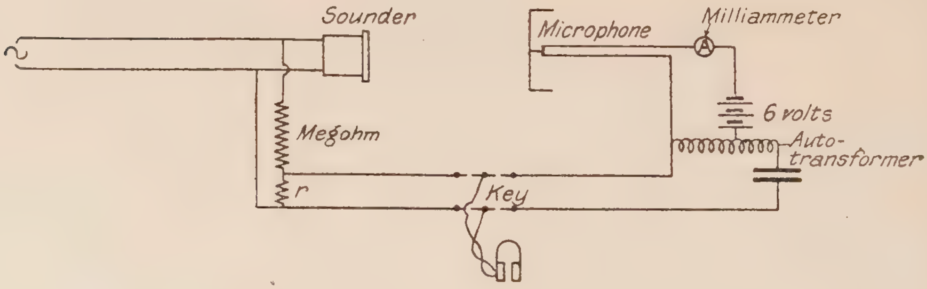
The ratio so obtained involves the constants of the detectors, and if the latter



(a) Shunts rr' adjusted till sounds are equal or extinguished (Magnetophone detector)



(b) Shunt r adjusted to extinguish sound. (Microphone detector)



(c) Shunt r adjusted till sounds are equal.

FIG. 3.—METHOD OF COMPARING DIAPHRAGM SENSIBILITY.

are then interchanged and their constants not altered in the process, it can be shown that the true ratio is the geometric mean of the two ratios obtained as above.

A method of measuring numerically the loudness produced in the listening

* *See* V. O. Knudsen, Phys. Rev., Vol. 21, No. 1 (1923). Also A. Stefanini, N. Cimento 26, Oct.-Dec. (1923).

telephone is illustrated in Fig. 3 (*c*) and consists in comparing the loudness with that produced by a portion of the sounder current tapped off as in a potentiometer.

It was not possible to obtain an absolute measure of the amplitude of the diaphragm, since the energy supplied to the telephone depends on the constants of

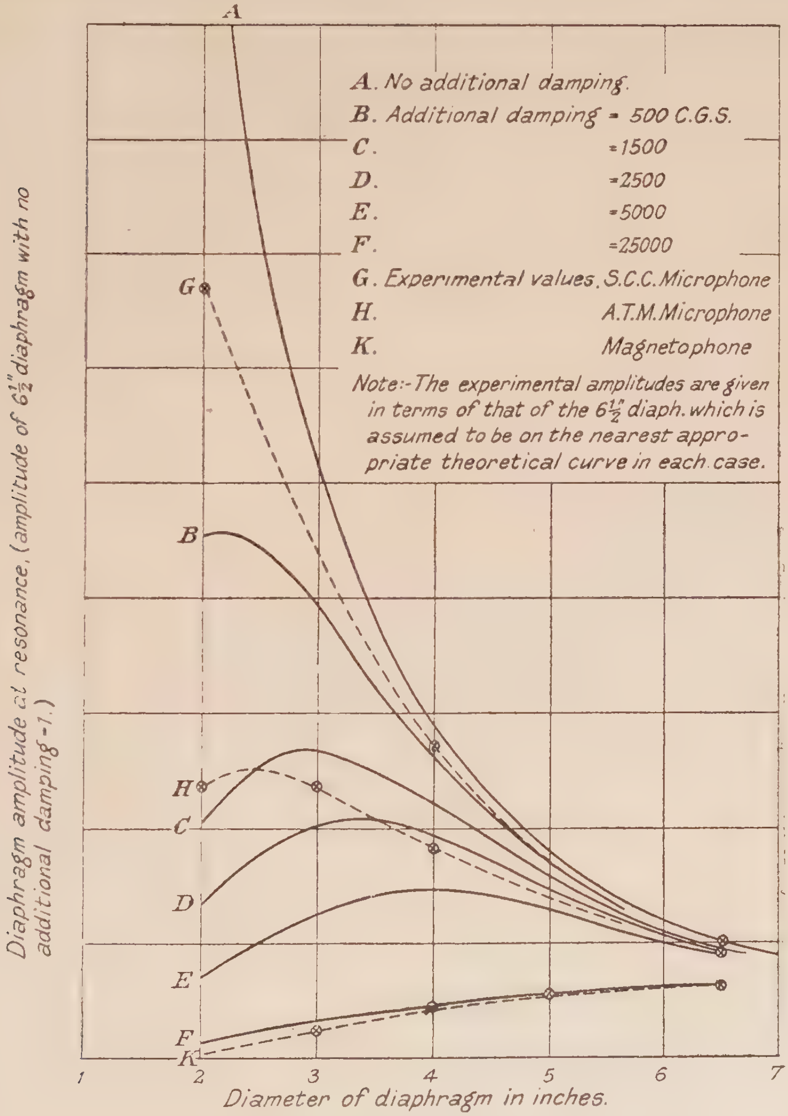


FIG. 4.—VARIATION OF SENSIBILITY WITH ADDITIONAL DAMPING.

the detector, but relative values were obtained by transferring the same detector to another standard diaphragm.

The condition that the constants of the detector should not alter in the process

of transference was difficult to obtain, and a considerable number of repetitions were necessary to obtain reliable average results. In the case of microphone detectors the resistance and sensitiveness vary with handling, or when the current is made or broken. These difficulties were partly reduced by bringing the resistance and current always to a definite value before taking an observation.

In all experiments where resonance curves were plotted, the diaphragms were held vertically and the sounder was placed with its diaphragm also vertical, at a constant distance of 6 ft. from it, and always in the same position in the tank.

The loudness of the sound heard was found to be very nearly uniform at all points in the tank, except within 1 ft. of the sounder diaphragm or near the walls.

Experimental Results.

Pure Sounds—Sensibility at Resonance.

According to the theory of the vibration of diaphragms of different dimensions, but of the same frequency, the amplitude of vibration at resonance for a given

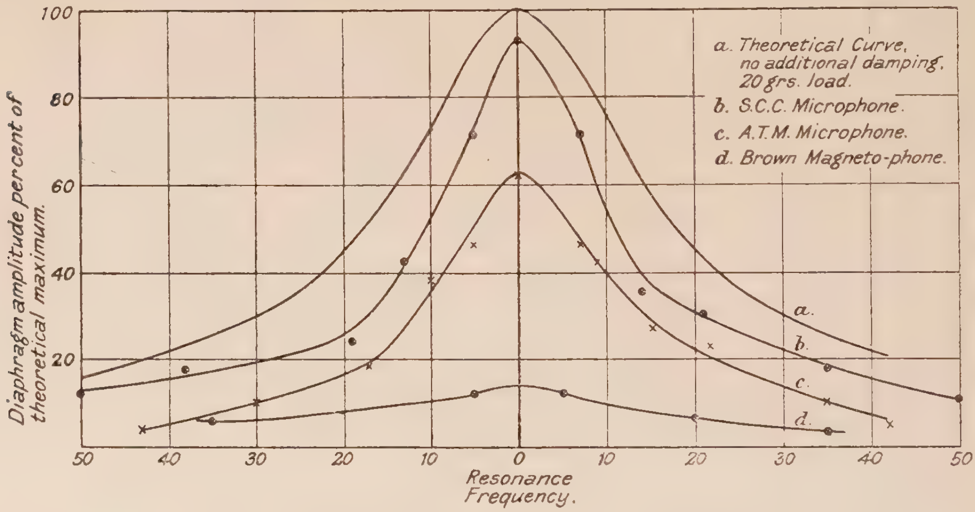


Fig. 5.—Resonance Curves for 4-in. Diaphragm.

excitation should be inversely proportional to the square of the radius of the diaphragm.

The results of experiments made to examine this deduction are given in Fig. 4. It will be seen that according to the theoretical deduction the amplitude at resonance of the 2 in. diaphragm should be about ten times as great as that of the $6\frac{1}{2}$ in. diaphragm, whereas the results show it to be only about twice as loud when the A.T.M. microphone is used as detector, and about six times with the S.C.C. microphone.

This difference can be readily accounted for by the fact that the simple theory takes no account of the extra damping due principally to the damping effect of the microphone which has to be added to that due to the water in contact with the diaphragm. It will be seen by comparing the typical theoretical and experimental resonance curves, Fig. 5, that, though the curves have a close similarity of shape,

the breadth of the experimental curves is different from that of the theoretical. The "breadth" of each curve was readily measurable, and the quantity "z" (equation 7) determined in each case, from which the value of the ratio B/A was calculable.

In order to account for the difference in the theoretical and experimental values of B/A, the possible causes of variation of the two factors were examined.

The value of A was modified to correct for the additional mass of the detector, as in the earlier calculations on the frequency of circular diaphragms in which this factor was involved. As these figures were found to agree closely with those determined experimentally, this value of A was considered as being correct.

The factor B may be modified either by additional damping, due to the detector, which increases its magnitude, or by the concentration of the energy of the sound waves in the tank, which reduces it.

These modifications were investigated in detail experimentally.

(1) *Damping Due to Detector.*—The damping of the vibrations of a diaphragm due to the attachment of the microphone was very marked. If a diaphragm were placed horizontally, it was found that any loose particles such as globules of mercury, drops of water or metal filings produced the same effect, and in an actual experiment a quantity of fine steel filings sufficient to fill three standard A.T.M. microphones, lying loosely on a small plate attached to the diaphragm at its centre, reduced the height of the resonance peak of a 4 in. diaphragm to one-third (using an A.T.M. microphone as detector).

In the case of the Brown telephone detector there was considerable electromagnetic damping (*see* Fig. 4). In order to reduce the microphone damping to a minimum, the special type of small single carbon contact (S.C.C.) microphone was devised (*see* Fig. 2), which consisted of a small block of carbon with a convex surface mounted on a pivoted lever (a fine steel needle), in contact with a flat block of carbon fixed rigidly to the centre of the diaphragm; the total weight of the whole microphone including the screw for attachment was barely 2 grammes, and its measured damping effect was about one-tenth that of an A.T.M. microphone.

Comparative resonance curves were obtained with these two types of microphone and with the Brown magnetophone, examples of which are given in Fig. 5 for a 4 in. diaphragm, and which show the relative damping effects very clearly. The slight variations in the mass of the detectors produce small variations in the breadth of the curves, but have no effect on the total height of the resonance peak.

It was also observed in the course of these experiments that detectors having loose mechanical parts produced very marked damping effects—and in particular devices of this kind designed on the lever principle to give greater amplitude to the microphone did not produce the desired magnification.

(2) *Energy Concentration.*—Owing to the fact that the diaphragm did not form part of an infinite rigid wall, and was not immersed in an infinite volume of water, the energy loss was very much smaller than calculated theoretically. Energy loss due to radiation of sound energy to the air, and to heating of the metal of the diaphragm was relatively negligible.

From the figures obtained for the breadth and height of the resonance curves it was found that the concentration effect was constant, and that the value B/A was reduced to one-half of its normal value, provided the diaphragm was not within a

few inches of the wall of the tank, or in the vicinity of another vibrating diaphragm (e.g., the sounder diaphragm itself) or any sound absorber such as an air cavity. Care was taken in all measurements to ensure that this was the case. Fig. 6 shows the result of an experiment in which the value of B/A was measured when a 2 in. diaphragm was brought close up to the sounder diaphragm, the two diaphragms being kept parallel. The increase in the damping was very marked at distances less than one foot, and was accompanied by a lowering in the pitch and amplitude. The proximity of a rigid wall tended to sharpen the maximum but the effect was not marked.

It was also observed that by suitably adjusting the position of a vibrating diaphragm, the damping of the sounder diaphragm could be increased or diminished, and its loudness diminished or increased accordingly.

In making the measurements of the breadth of resonances the diaphragms were fitted with a collar consisting of a circular sheet of iron $\frac{1}{8}$ in. thick, forming the base of a hollow cylindrical vessel in the centre of which the diaphragm was mounted, so as to lie flush with the collar. This collar was found to make no appreciable difference to the breadth of resonance in any but the case of the $6\frac{1}{2}$ in. diaphragms, where it increased the breadth by about 25 per cent., which would be anticipated, owing to the absence of a flat solid rim in this type of diaphragm.

A set of numerical values for the various factors is given in the following table, in which theoretical and experimental values are compared :—

Diam. of Diaph.	Type of micro-phone.	Theoretical values.			Assumed value of microphone damping, b.	$\frac{B+b}{2 \cdot 1 \times A}$.	Experi-mental value.	Relative amplitude at resonance.	
		B (reson-ance).	A	$\frac{B}{A}$				Theotl.	Exptl.
16·5 cms ($6\frac{1}{2}''$)	A.T.M.	78,000	374	208	5,000	105·8	103·2	1·0	1·0
10·15 cms. ($4''$)	A.T.M.	10,300	73·2	141	5,000	99·6	104·1	2·05	1·8
7·61 cms. ($3''$)	A.T.M.	3,260	27·8	96·6	5,000	141·5	131·8	2·14	2·4
16·5 cms. ($6\frac{1}{2}''$)	S.C.C.	78,000	374	208	500	100·0	93·2	1·0	1·0
10·15 cms. ($4''$)	S.C.C.	10,300	73·2	141	500	70·5	76·9	2·74	2·7
5·08 cms. ($2''$)	S.C.C.	655	8·2	78·5	500	66·3	62·8	6·46	6·7

Frequency of the diaphragm 850 ∾ ; all units c.g.s.

In the above table the values of A, B and B/A are obtained theoretically as described earlier in this Paper ; the quantity b, proportional to the damping due to the detector is added to B and its magnitude is such that the new values of $(B+b)/A$ are proportional to the experimental values of $2\pi z$.

The ratio of these values therefore gives the relation between the actual values of the energy dissipated in the water of a tank of finite dimensions, and the theoretical values in open water. It will be seen that this factor is almost exactly $1/2{\cdot}1$, and is constant for all diaphragms and detectors.

The *true* value for the microphone damping is therefore equal to $b/2{\cdot}1$.

The experimental values of B/A have all been corrected for the load due to the detector.

As a check on the accuracy of the value of $B+b$ the new amplitude of vibration was calculated in each case, assuming the amplitude to be proportional to $a^2/B+b$ (equation (4)). These values are given taking the amplitude of the $6\frac{1}{2}$ in. diaphragm

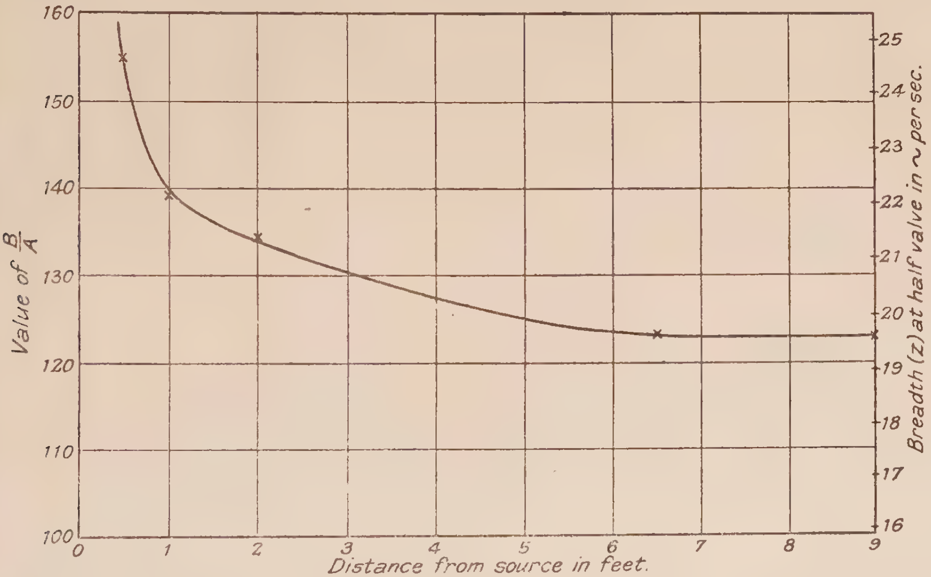


FIG. 6.—VARIATION IN THE BREADTH OF RESONANCE CURVE OF 2-IN. DIAPHRAGM WHEN APPROACHING SOUNDER DIAPHRAGM.

as unity for each detector, and the close agreement with the experimental values confirms the accuracy of the assumption.

In Fig. 4 is given a series of curves showing the effect of the damping due to the detector on the amplitude of vibration of different diaphragms, under the experimental conditions, the measured values being also plotted on the same graph. These show that the maximum sensibility is obtained when the microphone damping is equal to the water damping—and that in the case of an A.T.M. microphone, this damping is about equal to that of a $3\frac{1}{4}$ in. diaphragm with water on one side. This result is confirmed by the theoretical deductions of Mr. H. W. Hilliar, who carried out a mathematical investigation (unpublished) of the various damping effects on the vibration of a diaphragm, and is corroborated by actual experience at sea, where the "water" damping would be $2{\cdot}1$ times greater and the most sensitive diaphragm therefore $2{\cdot}8$ in. in diameter.

COMPLEX SOUNDS.

In the case of a noise consisting of sounds of various frequencies, in general away from the resonance region, the value of B is relatively insignificant and the maximum sensitivity is governed by the value of A. Here again the smaller diaphragms are more sensitive, but when a load is applied at the centre, the maximum sensibility occurs when the added load is equal to the effective mass A. The total added load in these experiments (except in the case of the $6\frac{1}{2}$ in. diaphragm, when the effect was relatively small) amounted to 20 grammes—roughly one-fifth the mass of a 3 in. steel diaphragm, which should therefore be the most sensitive type for use with this load.

This conclusion was confirmed by experiments, the results of which are given in

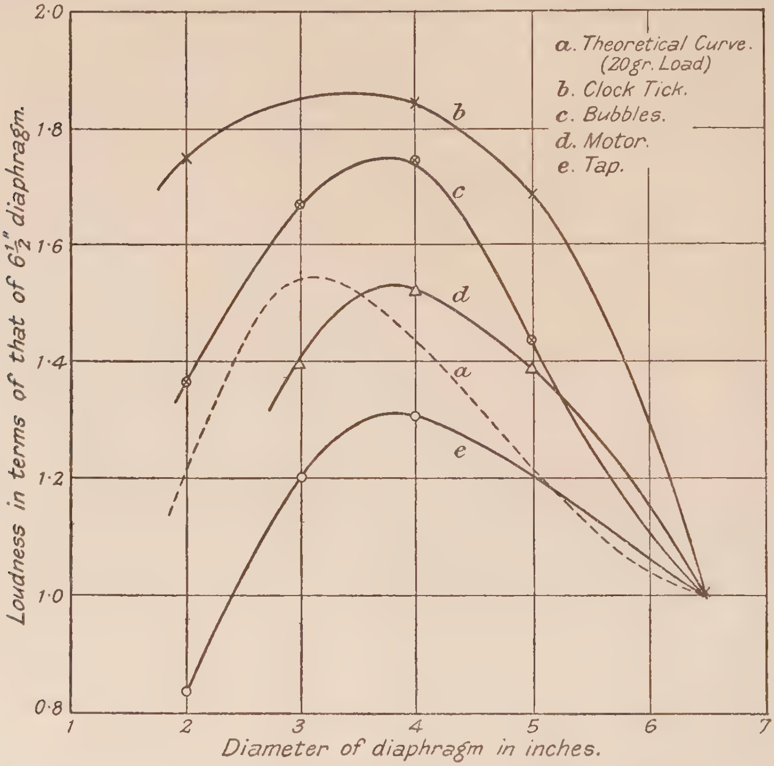


FIG. 7.—VARIATION OF SENSIBILITY TO COMPLEX SOUND. (A.T.M. MICROPHONE DETECTOR.)

Fig. 7. In considering these values it is important to notice that some of these sounds, particularly those due to the tapping and to the bursting bubbles were in the nature of "shocks," and that therefore on the cessation of the impulse the diaphragm resounded with its own frequency, hence in considering this "after-ring" the case of pure resonance arises again rather than that of forced vibration. This after-ring was observed to be much more persistent with a large diaphragm, and almost absent with a small one, thus confirming the excessive damping effect of the detector in the latter case. It should be remarked, however, that these noises,

though not true "complex-sounds," give a very good imitation of the sound produced by a vessel and since the ultimate object of the investigation was to produce the diaphragm most sensitive to sounds of this nature, the results obtained were of direct practical value.

This research was carried out during 1916 under the personal direction of Sir E. Rutherford, to whom the author has great pleasure in expressing his indebtedness.

The author also wishes to thank Dr. J. H. T. Roberts for his valuable collaboration in the experimental portion of this research.

This Paper is now published by permission of the Admiralty.

DISCUSSION.

Dr. F. L. Hopwood said that the Paper, which dealt with work carried out during the war but not previously released for publication, revived old memories, pleasant and otherwise. The author who had worked in a tank would have preferred the open sea, but workers in the open sea expressed a preference for tanks, while the speaker's lot had fallen to a reservoir, which combined the disadvantages of both. At the beginning of the war the experimental diaphragms ranged from 2 in. to 2 ft. in diameter, and the available theory indicated that the amplitude under the conditions discussed would increase indefinitely with decreasing diameter. Thin diaphragms in which the overtones were excited had also been tried. The damping produced by the detector was accounted for by the energy absorbed in the receiving system. The credit for finding the real optimum diameter was due to the work of the author and Prof. Lamb. No other authors have dealt with this subject in English, but the findings given in the Paper are supported by the German work "Unterwasser Schalltechnik."

Sir Richard Paget said that, like the previous speaker, he was moved to a reminiscent frame of mind by the Paper. Some idea of the progress made could be gathered from the fact that before the war the diaphragms which were used in connection with submarine bells used to be tuned *in air* to the required pitch ; the water damping would then put them out of tune by as much, possibly, as an octave. It was interesting to compare the diaphragms discussed with those found in Nature, in the ears of men and animals ; the diaphragm of an ear is of much smaller diameter, and the damping is much smaller than that due to a microphonic detector. It might be interesting to experiment with an unloaded diaphragm free from damping other than radiation damping ; this could be done by employing optical methods of detection. Prof. Rankine and others had very successfully used the human ear as a subaqueous detector.

Capt. C. W. Hume asked how far it was justifiable to express the damping due to the detector as an increment in the coefficient B in the resistance term of a differential equation for damped forced oscillations. The damping due to "loose mechanical parts" or to loose particles, to which the author refers in part (1) of his paragraph on this subject, would be rather in the nature of a succession of blows than a continuous function of $\dot{x}$, and it was difficult to believe that a granule inertia microphone would offer a resistance proportional to the velocity of the diaphragm. It was noteworthy that the curve for a "simple carbon contact" seemed to resemble the theoretical curve in shape far more closely than did the other curves.

Prof. A. O. Rankine said he ought to explain that the idea of using the human ear as a subaqueous detector had originated with Sir Richard Paget, who conducted his investigations while suspended in the sea by his feet. The speaker had preferred to adopt the more dignified posture normal to the human species. He did not consider the ear very sensitive to subaqueous sounds, but its damping was small in comparison with the damping due to the water.

Dr. E. H. Rayner said that the use of subaqueous bells had been mentioned. It is generally stated that the bells do not give out the notes they are supposed to give, the pitch being somewhat indefinite. How is it, therefore, that a diaphragm can be tuned to respond selectively to a given bell ?

Author's reply :—(To Dr. Hopwood) : The energy absorbed by the detector is undoubtedly used in exciting the listening telephone. The most sensitive microphone, therefore, produces the greatest damping effect. In the case of microphones this energy merely serves as a "trigger" for the release of a much greater amount of energy from a battery. In the case of the magnetophone, on the other hand, the whole of the energy is derived from the diaphragm, and for this reason the magnetophone damping is excessive, although it is a much less sensitive detector than

the microphone. No attempt was made to study the case of overtones, since it was found that the frequencies of the overtones of the diaphragms examined were not in agreement with Prof. Lamb's theory, probably on account of the presence of the boss and detector at the centre. In practice it was found that hydrophones with thin diaphragms in which the overtones were excited could not be reproduced in large numbers with any consistency, and thicker diaphragms were accordingly substituted in which the fundamental tone predominated. I have read the book referred to, which is by Lichte, and it was reading it which led me to communicate this Paper, even though the work described is eight years old, as it did not seem desirable that the only published experimental work on this subject should be in German. I have made no reference to it in the Paper, since this work was naturally quite independent, nor does the German work possess priority.

(To Sir Richard Paget) : An optical method similar to the one suggested has actually been used to examine the vibration of diaphragms. The work was done in 1919 by Dr. A. B. Wood, but is unpublished. His results are fully in agreement with those quoted in this Paper.

(To Capt. Hume) : The microphone depends for its action on the variation in the pressure on the carbon granules, which is never zero, since there is never a complete break in the circuit ; the damping is therefore continuous throughout the whole cycle. In the case of the S.C.C. microphone, when the vibration of the diaphragm becomes very violent, " chattering " of the contact begins, but this does not occur with small amplitudes. Mr. H. W. Hilliar, in the investigation referred to in the Paper, deals with the theory of damping very fully, and compares the microphone to an arrangement of carbon blocks which slide over one another frictionally, and his conclusions are in complete agreement with experiment.

(To Dr. Rayner) : The " bells " referred to in this paper are the standard Trinity House bells used for submarine signalling. They are specially tuned to a definite pitch in water, and it was found that diaphragms could readily be tuned to this frequency. A full description of these bells is given by Dr. Drysdale in the chapter on Submarine Signalling in " The Mechanical Properties of Fluids," where a drawing is also given.

VIII.—A DIRECT-READING FREQUENCY METER OF LONG RANGE.

BY ALBERT CAMPBELL, *B.A.*

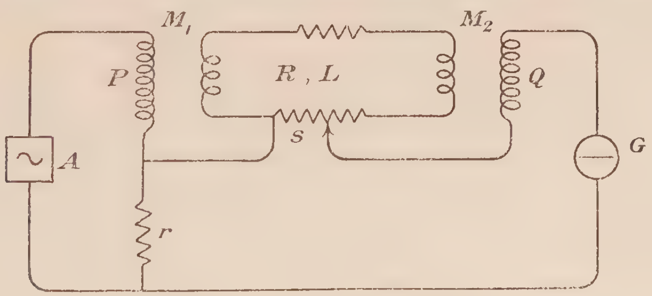
Received November 21, 1924.

ABSTRACT.

A direct-reading frequency meter for audio frequencies is described. It is a null instrument, reading by single adjustment, the working system embodying a new method of balancing mutual inductance by resistance. The standard type has five ranges, covering from 180 to 4,000 ∽ per second, with accuracy of the order of 1 in 1 000, and negligible temperature coefficient except at the lowest frequencies; an extra set of ranges from 18 ∾ per second upwards can be easily added.

THE accurate measurement of audio frequency is of importance, not only in acoustical experiments, but also in a great variety of electrical measurements; and it is often a most convenient method of determining speed of rotation in mechanical tests.

Perhaps the simplest method of measuring electrical frequency is the author's "wave-form sifter,"* in which a mutual inductance M is balanced against a capacitance C, or his Bridge Sifter, almost as simple, but better in working, in which a similar balance is obtained. But these methods, valuable as they are in many cases, have certain inherent disadvantages. For example :—



(*a*) Even the best condensers show considerable variation of capacitance with frequency and temperature; in the less costly types these defects may be accentuated.

(*b*) The condenser-mutual inductance (C, M) system does not easily lend itself to the design of an instrument with a number of ranges.

In spite of these disadvantages the author has found it possible to design C, M instruments which work well, but a better solution of the problem is based on a novel method of balancing mutual inductance against resistance. This is shown in the figure.

Two mutual inductances, M_1 and M_2, are connected with an alternating source A and a detector G (e.g., a vibration galvanometer or telephone) as shown, their

* Proc. Phys. Soc., Vol. 21, p. 69 (1907), and Vol. 24, p. 107 (1912).

secondary circuits, with added resistance, forming a loop having total self-inductance L and resistance R, of which a portion s is tapped off into the detector circuit; r is a non-inductive resistance. Let pulsatance $\omega=2\pi n$, where n is the frequency of the source. Then, when G shows no current,

$$\omega^2 M_1 M_2 = Rr \qquad (1)$$

and

$$M_1 s = Lr \qquad (2)$$

If we fix M_1, r and L we can set s once for all so as to satisfy condition (2). Then for any given frequency condition (1) can be satisfied by adjusting only M_2. Then

$$\omega^2 = Rr/M_1 M_2,$$

or

$$n \propto 1/\sqrt{M_2} = a/\sqrt{M_2}.$$

If r and M_1 are invariable, by changing only R we can obtain various values of the scale multiplier a.

In the actual instrument the scale of the variable M_2 is marked to read n directly for a given value of R, making $a=1$, and a number of other ranges are obtained by altering R in suitable proportions, giving simple scale multipliers.

It will be noticed that, unlike the C, M sifter types, which require double adjustment, the instrument reads by *single adjustment*.

If the resistance r has residual self-inductance l, and if there is direct mutual inductance m between P and Q, the conditions of balance become

$$\omega^2[M_1 M_2 - L(l-m)] = Rr \qquad (3)$$

and

$$M_1 s = Lr + (l-m)R. \qquad (4)$$

Now l is usually negligible; if not, it can be balanced by an equal small value of m.

In the instrument m can be set to zero, but as this may be altered by the effect of leads or other external circuits, a very minute adjustment is left for m, so that perfect sharpness of balance can always be ensured.

In the standard type of instrument the scale is so designed as to give an accuracy of reading of at least 0·25 mm. for 1 part in 1,000 (of n) at all points. There are five ranges (scale multipliers 0·3, 0·5, 1, 2 and 3), giving a total range of about 180 to 4,000 $\sim$ per second.

For the ranges from 600 up to 4,000 $\sim$ per second the temperature coefficient is negligible. It is very small for the 300 to 600 range, and slightly more for the lowest range. For the ranges from 600 to 2,500 $\sim$ per second the actual accuracy is in general within 1 in 1,000.

In the same instrument, by suitable variation of r and s an additional series of ranges can be provided, covering from 18 to 400 $\sim$ per second, the detecting instrument being a vibration galvanometer for the lower frequencies.

DISCUSSION.

Dr. E. H. RAYNER said it gave him great pleasure to hear once more from the Author, who had sought peace and quiet in Ireland after doing very much to put in order, in this country, those standards of measurement with which he was uniquely qualified to deal. He suggested that a letter of appreciation should be forwarded on behalf of the Fellows present. The precise measurement of audio-frequency, no less than that of radio-frequency, was becoming of the greatest importance in modern industry. In the United States the frequency of the supply

mains was used for time-keeping, and consumers were able to obtain very small neat clocks run as synchronous motors from the mains. The frequency was kept constant by the aid of a chronometric pointer at the generating station, a second pointer driven by a synchronous motor being mounted coaxially with this. The supply was controlled manually, so as to keep the second pointer in step with the first, giving a frequency of 50·000 ∿.

Dr. D. OWEN also congratulated the Author on his method. He had himself some years ago explored the possibilities of bridge methods of measuring frequency, but had failed to hit on a really satisfactory one. Mr. Campbell has achieved a fine success in the present meter. The two conditions of balance are quite independent, and the values of mutual inductance and resistance required are well within reasonable practical limits. For measurements on an impure source a thermionic oscillator might be desirable as an intermediary, using the beat method.

The PRESIDENT said he felt sure the Author would have been glad of a constant-frequency supply, such as that described by Dr. Rayner, had this been available in the laboratory which he had built (and plastered with his own hands) in Ireland. However, he had contrived to make time measurements to an accuracy of 0·01 per cent. with an antiquated grandfather clock. With the consent of those present, he would ask the Secretary to send a letter to the Author as Dr. Rayner had suggested. (Applause.)

Dr. ALFRED HAY (communicated): I wish to express my appreciation of Mr. Campbell's ingenious application of the principle of balancing a mutual inductance against a resistance to the construction of a new type of frequency meter. It is interesting to know that perfect sharpness of balance can always be ensured; this presumably applies only to practically pure sine waves, as it would appear that with a distorted wave silence could never be obtained. The new instrument appears to be capable of a very high degree of accuracy. Perhaps Mr. Campbell would state whether there is any likelihood of the adaptation of his instrument, with a suitable vibration galvanometer, to the measurement of low frequencies—from, say, 12 to 100. In my experience, available commercial instruments for the measurement of such frequencies are unsatisfactory.

Mr. S. BUTTERWORTH (communicated): The bridge recommended by the Author for the measurement of frequency is probably the best that could be devised. In addition to its long range, the second balance, which is always necessary in alternating current bridges, can be made to hold for all frequencies, so that in actual use only a single setting is necessary. This is a very great advantage. The Author makes no remark on the effect of want of quadrature in the mutuals. This will affect both equations of balance, and though unimportant at the lower frequencies, may be appreciable when the frequency is as high as 4,000 per second. The difficulty could be avoided by calibrating the system by known frequencies instead of relying on the electrical equation of balance. A more serious effect of the want of quadrature would, however, be a lack of sharpness of balance at the higher frequencies, as the second balance is not quite independent of frequency when the impurity of the mutuals is taken into account. It is possible, however, that by a judicious choice of the mutuals this effect might be made negligible, as the two impurities can exert mutually cancelling effects in the equation of balance.

Dr. E. T. PARIS (communicated): The direct-reading frequency meter described by Mr. Campbell appears to be an instrument which should have many applications to acoustical experiments. The range mentioned in the Paper for the standard type of instrument is 180 to 4,000 ∿ per second, and while this should be sufficient for most needs, there are some branches of acoustical work in which the frequencies to be measured are generally below 180 ∿ per second. It would be of interest to know if there is likely to be any difficulty in the manufacture and operation of a meter of this type for a range of (say) 50 to 100 ∿ per second.

Mr. B. DAVIES (communicated): The first production to issue over the threshold of Mr. Albert Campbell's private laboratory is a novel frequency meter. One is glad to see it. Like all his work at the N.P.L., and that of his colleagues who have been responsible for the remarkable development of standardization in this country, this meter goes forth with the usual high credentials. To dispose of variable inductance or variable capacitance or both as the basis of frequency measurement is a great step forward and a very neat one, eliminating as it does obvious inherent difficulties as well as difficulties in manufacture. It seems to me it were well if Mr. Campbell allowed two types of his instrument to find their way to the market. One, a standard type of high accuracy in tune with Mr. Campbell's own heart, and another, at a lower price, possessing moderate accuracy, to be used as a hack instrument in the rough and tumble work that goes on in most laboratories for purpose of approximate computations. This

latter, it is well understood, is against Mr. Campbell's grain ; but if he can be prevailed upon to consider this heretical suggestion from the point of view of the "common weal," whose interests he has persistently at heart, we may witness a concession ! There is a demand for both types.

Mr. ROLLO APPLEYARD (communicated) : To realise the importance of Mr. Albert Campbell's results it has to be remembered that the direct measurement of frequency within the required range necessitated the invention of a method, and—what in some respects was more difficult—the design and construction of an instrument capable of satisfying the conditions that govern such determinations. His success in this two-fold task will be appreciated by all physicists. In net-works of the kind involved in this problem, the conditions for balance have always to be represented by two equations, relating respectively to the "real" and to the "imaginary" parts of the solution ; and corresponding to these equations there must be two adjustments. The advantage of the net-work devised by Mr. Campbell is that the two equations are simple, and that one of them may be satisfied by a permanent adjustment. He thus fulfils, without complexity, the direct-reading conditions. Another method of measuring frequency and of relating the other factors in the network may be sought by adjusting them to give no current in the second mesh (of RL), instead of no current in the third (of QG). The required conditions for no current in the second mesh I find to be

$$M_1 L_3 \omega^2 = r_1 s \qquad \text{(1B)}$$

and

$$M_1(r+R_3+s) = rM_2 \qquad \text{(2B)}$$

where L_3, R_3 denote the self-inductance and resistance of the third mesh. The relations of (1B) and (2B) may enable check-readings to be obtained by transferring G from the third to the second mesh. Also (2B) expresses the ratio of the two mutual inductances in terms of resistances for the case when (1B) holds. On the other hand, for the purpose contemplated, it might imply a permanent adjustment between M_1 and M_2, and in that event it would lead to a less convenient arrangement than that embodied in Mr. Campbell's instrument.

AUTHOR'S reply (communicated) :—

(To Dr. Alfred Hay and Dr. E. T. Paris) : The standard type of the instrument is designed to cover the range of ordinary telephonic frequencies. By a slight elaboration it is easy to furnish in the same instrument a series of lower ranges from 18 up to 400 $\sim$ per sec. It is only necessary to reduce r and s in the same proportion (here 100 : 1). These lower ranges have been found to work very well with a vibration galvanometer as detector.

(To Mr. S. Butterworth) : In the design of the instrument the possibility of errors at the higher frequencies due to impurity in the mutual inductances was carefully kept in mind. Careful tests have shown that with coils of fairly simple type the actual errors lie within about 1 part in 1,000 up to frequencies of 3,200 $\sim$ per sec. If specially high ranges are wanted, the impurities in the inductances can be reduced by subdividing the coils in any of the well-known ways. In general the instrument follows the equation of balance so accurately that it can be accurately calibrated without the use of any known frequencies.

THE PHYSICAL SOCIETY OF LONDON

AND

THE ROYAL METEOROLOGICAL SOCIETY

A DISCUSSION

ON

Ionization in the Atmosphere and its Influence on the Propagation of Wireless Signals

Held November 28, 1924

AT THE

IMPERIAL COLLEGE OF SCIENCE, SOUTH KENSINGTON, S.W.

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CONTENTS.

Opening Address by Dr. W. H. Eccles, *F.R.S.*

MODERN wireless signalling came into being when Oliver Lodge showed the British Association at Oxford in 1894 that dots and dashes could be carried by Hertzian waves across a distance of 150 yards and received by a coherer which was automatically re-set after each response. In the following year the Russian physicist, Popoff, used a long vertical wire and a smaller automatic coherer for recording lightning flashes. Thus the warning that atmospheric electricity was going to interfere with the new-born art of wireless telegraphy followed quickly upon its inception. Before another year was over Rutherford had sent signals half-a-mile across Cambridge, and Marconi had improved and invented many pieces of apparatus for making electric waves and receiving them, and had developed a complete plant for wireless telegraphy that already showed commercial possibilities. But he also quickly found that lightning flashes, near and distant, produced records on his morse tape and sometimes obliterated messages. These unwanted signals produced by natural electric waves were called X's or strays. They have also been called atmospherics, statics, sturbs and other names. They are still with us and are still a terrible nuisance, although nowadays our apparatus is less susceptible to them. They can often be heard in the telephones of broadcast listeners where they appear sometimes as sharp clicks, sometimes as grinding or crashing noises, and sometimes as frying or hissing sounds. They are distinctly connected with meteorological conditions, and therefore fall within the scope of this evening's discussion.

Within two or three years of the events just mentioned, Marconi had sent messages to distances of 20 or even 40 miles in spite of intervening hills. Did the electric waves go through or over the hills? Captain Jackson* (now Admiral Sir Henry Jackson) settled this question by comparing signals received in a ship close to a mountainous island with those received well beyond. He found there was a distinct shadow which proved that the waves went not through but over the hills by the process of bending called diffraction.

But when in 1900 Marconi sent signals across the Atlantic, thus overleaping a hill of salt water 130 miles high, it was felt that something more than diffraction was required to explain this event. Heaviside's † suggestion that the waves might be guided round the globe by a conducting layer in the upper atmosphere seemed very promising, though there were many objections to it. This ad hoc layer was given the name of the Heaviside layer and produced a crop of speculation. For instance, Erskine Murray ‡ pointed out that the wave front from a sending station would spread over the globe as an expanding ring until its diameter was 8,000 miles, and then

* Proc. Roy. Soc., Feb. (1902).
† Ency. Britt., 10th edition, Vol. 33, page 215.
‡ Inst. Ell. Engineers, Dec. (1905).

would travel onwards as a contracting ring until it reached the Antipodes, when, if the absorption had been small, the signals would be very strong indeed. But speculation was much disturbed by Marconi's* discovery in 1902 that a ship crossing the Atlantic could receive signals four or five times as strong by night as by day. The Heaviside hypothesis could not deal with this new surprise.

It was obvious, however, that the daily increase of opacity might be connected with ionization of the air by the suns rays. A gas is said to be ionized when it contains electrified particles of molecular or smaller order of magnitude. The smallest ions are particles of negative electricity devoid of matter and are called electrons; the next smallest are single atoms or molecules of gas carrying positive or negative charges of electricity. The question arose how could the presence of these ions in air affect the propagation of electric signal waves? No clear suggestion was made until 1912,† when it was shown by mathematical reasoning that an electric wave moving through air containing ions makes the ions vibrate with the result that the velocity of propagation is increased—somewhat as waves in water have their velocity altered by the presence of fine mud in suspension or of innumerable minute air bubbles. An immediate deduction from the known fact that the ionization of the air is greater in the upper strata than in the lower is that the waves will follow downward bending trajectories rather like rifle bullets. Evidently there will be two sharply marked cases, one in which the ions are electrons, and the other in which they are of molecular size and therefore several thousand times heavier. It was suggested in 1912 that the Heaviside layer was a layer so high and rarefied that free electrons could exist in it and that the absorption of energy was negligible, that its lower surface was fairly sharply defined, and that the layer therefore bent electric waves of all lengths downwards so sharply as to simulate the act of reflection from a mirror. Accepting all this the waves from a sending station would run under the dome formed by this permanently ionized layer much as sound waves run under the dome of St. Paul's from the Whispering Gallery. It was also suggested that in contrast with the Heaviside layer certain layers underneath it became ionized daily by the sunlight, and that as these ions are relatively heavy the bending of the wave trajectory would depend upon the wavelength and show daily variations. It ought to be mentioned here that Fleming‡ has suggested that the variation of inductivity of the air with height might be an aid or an alternative to ionic refraction.

As information accumulated during the succeeding years these hypotheses of the effect of the ionization of the atmosphere received increasing confirmation. For instance, as was pointed out§ in 1920, in the rarefied atmosphere where electrons exist and the mean free path is great, the plane of polarization of electric waves will be rotated because the agitated electrons move in the earth's magnetic field. To this cause may be attributed the errors that arise in applying direction finding apparatus by night. Again, the recent successes of amateurs in communicating with Australia and New Zealand with short waves during certain of the dark hours affords additional support. The fact that many of the successful amateurs utilize a series condenser in their transmitting antenna in order to avail themselves of the

* Proc. Roy. Soc., June (1902).
† Proc. Roy. Soc., June (1912).
‡ Proc. Phys. Soc., August (1914).
§ Inst. Ell. Engineers, December (1920).

upwardly directed radiation so obtained has been frequently emphasised at recent meetings of the Radio Society of Great Britain and is corroboration of the existence of the Heaviside layer. Again, experimenters in England and France have deliberately aimed waves at the sky by means of parabolic reflectors and have been enabled to transmit to great distances. It seems not unlikely that very short waves, less than 10 metres long, may prove to be of great practical value by day as well as by night in the early future. But all this confirmatory information would not be satisfying if it were not for the fact that other branches of meteorological and geophysical science are bringing independent evidence of the existence of these conducting layers in the atmosphere. To some of this evidence, as well as to other radio telegraphic evidence, we are gathered to listen this evening.

ATMOSPHERIC IONIZATION AND ITS VARIATIONS.

By C. Chree, *Sc.D., LL.D., F.R.S.*

Abstract.

The atmospheric electricity elements dealt with include the potential gradient, the numbers of positive and negative ions and the charges carried by them, the conductivity of the air and the vertical air-earth current. For instantaneous values it suffices to know the potential gradient and either the conductivity or the current. But in the case of mean values, depending on a number of individual observations, a knowledge of all three elements is necessary. Particulars are given of the annual variations observed at various stations. Information as to diurnal variation is very scanty, except in the case of potential gradient. The monthly values in the annual variation, and the hourly values in the daily variation, are expressed as percentages of their arithmetic means. This is done partly because less uncertainty attaches to relative than to absolute values in the case of all the electrical elements. Reference is made to some conclusions as to the diurnal variation of potential gradient and the existence of a sun-spot period recently arrived at by Dr. Bauer and Dr. Mauchly, of the Carnegie Institution of Washington.

§ 1.

THE fact that the earth's atmosphere is ionized was really discovered before anything was known about ions. It was long supposed that the loss experienced by all charged bodies arose from defective insulation in the supports, but eventually it was discovered that defective insulation accounted for only part, sometimes a very small part, of the loss. For historical details the recent work by B. Chauveau* may be consulted. I have derived considerable assistance from Parts II and III of this work in making the following compilation.

Amongst the earlier investigators Elster and Geitel—regarded in their day as the Castor and Pollux of atmospheric electricity—were pre-eminent for their zeal. They devised apparatus for measuring "dissipation," as it was called, and the radioactive contents of the atmosphere. Most of the earlier observations were made with their instruments. The results obtained, unfortunately, are difficult to interpret, and no use is made of them here. Most of the results now available as to the ionic contents of the atmosphere have been obtained with the Ebert apparatus, which is in a convenient form for use. Here, again, unfortunately some suspicion attaches to the results. The instrument was accused a good many years ago by Prof. W. F. G. Swann† of underestimating the number of negative ions. Observations and experiments made by E. H. Nichols‡ appeared to show that if the defect described by Prof. Swann existed, it was trifling under the ordinary conditions of the Kew observations. Prof. Swann adhered, however, to his original views, and they seem to derive support from a more recent investigation by H. Norinder.§

The ions which the Ebert apparatus is primarily intended to catch are those now known as light ions, with mobilities in the neighbourhood of 1·5 (i.e., under a

* Electricité Atmospherique, Paris Libraire Octave Doin (1924).

† Terrestrial Magnetism, Vol. 19, p. 205.

‡ Terrestrial Magnetism, Vol. 21, p. 87.

§ Stockholm Arkiv För Mathematik, Astronomi och Fysik, Band 15, No. 2.

potential gradient of 1 volt per centimetre they have a velocity of about 1·5 cm. per second). If it is assumed that all the ions caught are of one species, a second observation of a modified kind with the Ebert apparatus enables the mobility to be calculated. What the first observation really gives is the charge of free electricity —positive or negative as desired—per cm.³ of the atmosphere on ions caught by the instrument. Assuming each ion to carry a definite charge, the number of ions positive or negative can be calculated. In the earlier observations taken with the apparatus the ionic charge was assigned the value $3{\cdot}4 \times 10^{-10}$ E.S.U. originally found by Sir J. J. Thomson. The value obtained more recently by Prof. Millikan, $4{\cdot}8 \times 10^{-10}$ E.S.U., has now come into general use. It is not always easy to ascertain definitely what value has been used.

§ 2.

It is now known that the heavy ions discovered by Langevin may have to be taken into account. Our information about them is largely dependent on observations made in Ireland by McClelland, Kennedy, Nolan and others. According to the Irish experiments, the mobility of these heavy ions is only about $\frac{1}{3000}$ of that of the light ions, and if they were not more numerous than the light ions their influence on the conductivity would be negligible. They appear, however, often to be much more numerous. For the mean numbers n_1 and n_2 of the positive and negative light ions per cm.³ observed at a number of stations, Chauveau* gives

$$\text{On land, } n_1=750,\ n_2=630,\ q'\equiv n_1/n_2=1{\cdot}20.$$
$$\text{At sea, } n_1=730,\ n_2=580,\ q' \qquad =1{\cdot}25.$$

But at Dublin McClelland and Kennedy† found the number of heavy ions to vary between 3,700 and 60,000 per cm.³, the mean number being about 16,000.

The Ebert apparatus, as ordinarily used, catches only a small percentage of the heavy ions; but when they are as numerous as at Dublin, even a small percentage becomes important. At Dalkey, in the neighbourhood of Dublin, McClelland and Kennedy found usually less than 1,000 heavy ions per cm.³. They also found the number of light ions in Dublin to be small compared with the number at Dalkey. The presumption is that the number of heavy ions is largely determined by the amount of dirt in the atmosphere. If light ions ceased to be light ions by simply attaching themselves to dust, unless dust particles are all of one size, we should expect the resulting heavy ions to have a variety of mobilities. Pollock‡ claimed to have observed at Sydney ions of mobility intermediate between the ordinary light and heavy ions. But the heavy ions observed in the natural atmosphere at Dublin make apparently at least an approach to uniformity. In any case, what seems to happen is that in the dirt-laden air of large towns there is a smaller number than usual of light ions. A larger share of the vertical current, which is continually passing between the earth and the upper atmosphere, has to be carried by heavy ions of low mobility. The electrical conductivity falls, and the potential gradient goes up. In a recent letter to "Nature,"§ Prof. Nolan

* l.c. Troisieme Fascicule, p. 89.
† Proc. Roy. Irish Acad., Vol. 30, Section A, p. 72, &c.
‡ Proc. Roy. Soc., N.S.W., p. 61, &c.
§ Nature, Vol. 113, p. 493 (1924).

illustrated the rise in the potential gradient observed in a number of cases at Dublin with increase in the number of heavy ions. The fact has been observed at a good many stations that potential gradient tends to be higher on dusty or hazy than on clear days. This question was investigated in considerable detail some years ago at Kew Observatory,* use being made of records of visibility. With the setting up at Kew of an Owens' atmospheric-pollution recorder in 1921, a more precise investigation became possible. The practice at Kew has been to measure the potential gradient at each hour on 10 selected fine-weather days each month. Dividing these days for each month of the years 1921 and 1922 into five days of less and five days of greater atmospheric pollution, the following mean results were obtained† :—

Clean days.	Dirty days.
Dirt, 0·252 milligram per metre³.	Dirt, 0·490 milligram per metre³.
Potential gradient, 252 v./m.	Potential gradient, 343 v./m.

The inference was drawn that if the effect on the potential gradient were a linear function of the dirt, then, if the atmosphere had been perfectly clean, the mean potential gradient for the two years would have been only about 150 v./m., instead of about 300 v./m. as observed. Along with the rise in potential there went a fall in conductivity,‡ but the information on this point is much less complete.

Part only of the dust in the atmosphere is the work of man. In some places, and at some seasons, Nature is a great dust producer. Still, the fact remains that if the stations where atmospheric electricity results have been obtained had all been in the country, instead of being mostly urban or suburban, our information would have been more truly representative than it is.

§ 3.

Returning to instrumental questions, it may be added that most of the results at sea have been obtained from observations made with special apparatus on board the magnetic survey vessels of the Carnegie Institution of Washington.§ On land use has also been made of an apparatus designed by Dr. C. T. R. Wilson.‖ With it direct measurements can be taken of the potential gradient and the air-earth current, whence the conductivity can be calculated. A variant of this apparatus was used by Lutz¶ at Munich.

In the case of the Ebert apparatus the positive and negative ions and the conductivities for which they are responsible are measured separately. The free charges, positive and negative, per cubic *metre* corresponding to Chauveau's mean land values $n_1=750$, $n_2=630$ are respectively

$$E_1=750\times10^6\times4{\cdot}8\times10^{-10}=0{\cdot}36 \text{ E.S.U.}$$
$$E_2 \qquad\qquad\qquad =0{\cdot}30.$$

* Proc. Roy. Soc., A, Vol. 95, p. 210.

† Proc. Roy. Soc., A, Vol. 105, pp. 321 and 323.

‡ cf. Nature, Vol. 113, p. 856 (unchanged or diminished current, when gradient is higher, implies lower conductivity).

§ Carnegie Institution of Washington, Dept. Terrestrial Magnetism, Researches, Vol. 3, pp. 361 *et seq.*

‖ Proc. Camb. Phil. Soc., Vol. 13, p. 184.

¶ Sitzungsberichte, K. Bayer., Akad. der Wiss. Math.-physik Klasse, Jakrgang, 1911, p. 329.

If we prefer electromagnetic units, and charges per cm.^{3}, we have

$$E_1=1{\cdot}2\times10^{-16} \text{ coulombs per cm.}^3.$$

If n_1, n_2 be the numbers, k_1 and k_2 the mobilities of the free positive and negative ions, all supposed of one class, the air-earth current, assuming the air itself at rest, is given by $i=(k_1n_1+k_2n_2)eP$, where e is the ionic charge and P the potential gradient (measured, of course, in the same units as the other quantities).

As an example, suppose

$$n_1=n_2=500,$$
$$k_1=k_2=1{\cdot}5 \text{ cm./sec. for 1 v./cm.},$$
$$P=100 \text{ v./m.}$$

Using E.M.U. throughout, we have

$$k_1=k_2=1{\cdot}5\times10^{-8},$$
$$P=(100/100)\times10^8,$$
$$e=1{\cdot}6\times10^{-20},$$

and so $i=1{,}000\times1{\cdot}5\times1{\cdot}6\times10^{-20}=2{\cdot}4\times10^{-17}$, or $2{\cdot}4\times10^{-16}$ amp. per cm.^{2}.
As an example of electrostatic units, taking the data as above, we have

$$k_1=450, \text{ as the volt is } 1/300 \text{ of the electrostatic unit},$$
$$e=4{\cdot}8\times10^{-10},$$

and the conductivity λ_1 from positive ions is given by

$$\lambda_1=150\times500\times4{\cdot}8\times10^{-10}=1{\cdot}1\times10^{-4} \text{ E.S.U.}$$

§ 4.

Table I. contains a variety of results for the annual variation, and Table II. a few data for diurnal variation. The monthly and hourly values are shown as percentages of their mean. This is done partly with a view to bringing out resemblances or differences between the several elements and the different stations, partly because percentage values may remain correct and useful even when large uncertainties attach to absolute values. For example, in the case of P, so long as the factor for reduction to the infinite plane is constant, the percentage values are unaffected by an error in the factor.

Thanks, in the first instance, to Lord Kelvin's invention of the water-dropping electrograph, most stations concerned with atmospheric electricity have a continuous record of potential gradient, and most of the data for P in Table I. are derived from electrographs. Potsdam and Davos (Switzerland) are the only two stations which had continuous—or quasi-continuous—records of any other element. The apparatus in use at both stations gives a measure of the conductivities λ_1 and λ_2 due to positive and to negative ions separately, by measuring the loss of charge in a body charged alternately positive and negative. When the conductivity and potential gradient are known, the air-earth current can be calculated. The earlier of the two sets of data for Potsdam were derived by K. Kähler* from 12 months, October, 1910, to September, 1911. The later refer to the years 1912 to 1921, and are derived from a recent Paper by H. Markgraf.† The results for Davos are for the year December,

* Ergebnisse der Meteorolog. Beob. in Potsdam, im Jahre 1911, p. xvi.
† Meteorologische Zeitschrift, Vol. 41, p. 65.

TABLE I.—*Annual Variation of Electrical Elements (Percentages of Annual Mean).*

Place.	Element.	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Annual Mean.
Kew (A) ...	P	**145**	131	122	106	82	60	**59**	60	75	96	128	136	333 v/m
(B) ...	P	147	139	119	102	72	**46**	53	53	73	100	**160**	138	299 v/m
,, ...	E_1	**72**	81	98	93	118	**135**	114	134	108	96	79	**72**	0·155 E.S.U. (per m^3)
,, ...	E_2	**68**	76	103	105	118	126	117	**138**	107	79	85	74	0·114 E.S.U. (per m^3)
,, ...	i	82	89	96	105	110	107	99	111	123	**129**	**74**	75	$0{\cdot}78\times10^{-16}$ amp/cm^2
(C) ...	P	**165**	143	107	81	58	53	**44**	82	80	101	125	**165**	255 v/m
,, ...	E_1+E_2	70	50	80	125	123	126	**182**	135	115	81	67	**46**	0·37 E.S.U. (per m^3)
,, ...	λ	44	58	74	128	133	140	**173**	147	106	90	63	**43**	$0{\cdot}27\times10^{-4}$ E.S.U.
,, ...	i	97	102	**84**	104	100	90	104	**115**	108	110	100	85	$0{\cdot}54\times10^{-16}$ amp/cm^2
Tortosa	P	133	96	112	91	92	**80**	84	93	92	100	112	**116**	102 v/m
	λ	97	102	105	**107**	100	100	99	96	104	102	96	**92**	$1{\cdot}80\times10^{-4}$ E.S.U.
	i	**122**	108	110	101	87	**78**	82	85	99	100	111	118	$2{\cdot}03\times10^{-16}$ amp/cm^2
Potsdam (A)	P	136	110	98	111	89	**69**	71	93	72	99	109	**144**	254 v/m
,,	λ	**74**	81	109	102	114	119	**121**	102	107	97	89	84	$0{\cdot}95\times10^{-4}$ E.S.U.
,,	i	101	89	105	110	101	89	89	97	**84**	105	101	**127**	$2{\cdot}37\times10^{-16}$ amp/cm^2
,,	λ_2	**70**	82	111	98	109	123	**125**	105	109	98	89	82	$0{\cdot}44\times10^{-4}$ E.S.U.
,,	i_2	98	89	110	107	98	92	92	98	**86**	107	101	**122**	$1{\cdot}09\times10^{-16}$ amp/cm^2
(B)	P	**146**	143	107	92	79	75	**72**	74	79	87	109	137	202 v/m
,,	λ_2	90	**75**	99	107	112	107	**115**	**115**	104	106	92	78	$0{\cdot}45\times10^{-4}$ E.S.U.
,,	i_2	**135**	102	104	100	93	**85**	92	**85**	90	97	104	114	$0{\cdot}98\times10^{-16}$ amp/cm^2
Davos	P	**152**	140	136	102	88	**52**	71	73	74	71	98	146	69 v/m
	λ	**58**	81	77	96	109	**140**	115	121	118	127	99	61	$2{\cdot}68\times10^{-4}$ E.S.U.
	i	87	**125**	109	110	108	**80**	90	98	95	101	105	93	$1{\cdot}71\times10^{-16}$ amp/cm^2
Munich	P	**157**	131	140	60	49	**48**	84	82	84	64	154	146	1·88 v/m
	i	**159**	128	102	86	89	68	**65**	88	119	77	94	128	$1{\cdot}02\times10^{-16}$ amp/cm^2
Strelitz	E_1	88	**84**	100	105	90	96	107	**112**	111	111	105	91	0·340 E.S.U. (per m^3)
	E_2	85	**83**	105	111	88	97	105	**113**	110	109	104	89	0·327 E.S.U. (per m^3)

1909, to November, 1910. The original discussion by Prof. C. Dorno* I have only seen in abstract; the data given here are derived from a review of Dorno's work by K. Kähler.†

The results as to charges, conductivities and current at the other stations are from eye readings and refer only to a short period of the day, about 15 h. at Kew, 11 h. at Tortosa, and noon at Munich and Strelitz. The hour of observation was presumably dictated by the general convenience, and there is no reason to suppose that the values observed represent the mean value for the day.

In the case of Kew the (A) values for P are from hourly curve readings on fine weather days, for 11 years 1912 to 1923. The (B) data, on the other hand, all refer to 15 h., when P is normally below its mean value. The results for P are from the selected fine weather days, 10 a month. The other results are derived from all the days of absolute observation, representing on the average about half the days of the year, no observations being taken when the weather is wet or very stormy. The Kew (C) results are from data given in a Paper by G. Dobson.‡ These results, including those for P, are from eye observations with the Wilson apparatus near 15 h. during the years 1909 to 1912. The results for i for August, 1911, were abnormal and have been omitted, as this had apparently been done by Dobson.

The Tortosa§ results are means from the years 1918 to 1923. The mean value of P for the day from the years in question differed very little from that at 11 h. The values obtained for λ and i at Tortosa were derived by the Gockel-Schering method. The results for Munich, due to Lutz,‖ represent the results of eye readings on fine weather days from an instrument similar in type to Dr. Wilson's. The results for Strelitz are from a recent Paper by S. Wiedenhoff.¶ They represent three years' observations, extending from August, 1920, to July, 1923.

In Table I. the highest and lowest values are in heavy type. The annual variation of P is conspicuous at all the stations; P is high in winter and low in summer. The ionic charges and the conductivity vary on the whole in the opposite direction to P, being highest in summer. At Tortosa, however, the conductivity seems little dependent on the season, and consequently the vertical current shows a large fluctuation similar to that in P, the highest values occurring in winter. A similar phenomenon appears at Munich, and also in the (B) results from Potsdam for i_2 (i.e., the current carried by the negative ions). In the (A) results for Potsdam, the mean values for i_2 and i from the four summer months May to August, are only slightly below the means for the twelve months; while at Kew, in both groups of years, the mean value of i for the four summer months is somewhat above the mean for the year.

How far the diversity in the results from the several stations arises from the difference in the hours of observation, or represents an accidental feature of the years selected, it is impossible to say. The general conclusion is that of the three elements, potential gradient, conductivity and vertical current; the last-mentioned is the least variable.

* Studie uber Licht und Luft des Hochgebirges, Vieweg & Sohn (1911).
† Meteorologische Zeitschrift, Vol. 47, p. 72.
‡ Metl. Office, Geophysical Memoirs, No. 7.
§ Bol. del Observatorio del Ebro, Vols. 9 to 14.
‖ loc. cit. Tab. 2, p. 346.
¶ Met. Zeit., Vol. 41, p. 72.

§ 5.

In Table II. the Potsdam data correspond to the (A) series in Table I., and the Davos data are also for the same epoch as in that Table. At these two stations the entry under an hour really applies to 60 minutes *ending* at that hour. The Kew and Eskdalemuir results are from eye readings taken in 1914 by E. H. Nichols* and other members of the observatory staffs whose co-operation he secured. At Kew there were in all 12 days of observation, four from each month specified. For Eskdalemuir only six or seven days were available.

The potential gradient at Potsdam and Kew—and the same is true of many other stations—has a double daily oscillation, with minima in the early morning and afternoon, the latter increasing in prominence in summer. At Eskdalemuir, in summer, the morning minimum is hardly represented, and the fall to the afternoon minimum begins exceptionally early.

At the two British stations the conductivity and the potential gradient clearly vary in opposite directions, and the change in i is relatively small, but the days of observation were too few to secure smooth results. At Potsdam the current appears as variable as the conductivity, but the variations in both are comparatively small. The results for Davos in Table II. are for λ_1 only, but the total conductivity λ has a very similar diurnal variation. For the year as a whole, λ is much higher by night than by day, and according to Kahler† it is on the average about thrice as large as at Potsdam. Davos is about 1,550 metres above sea level. It would obviously be of interest to know whether it is fairly representative of high-level stations. The diurnal variations of potential gradient and air earth current, as well as conductivity, for the year as a whole at Davos are shown diagrammatically in a recent work by Dorno.‡ The potential gradient has a double oscillation, the forenoon minimum being much the more prominent. The air-earth current does not vary much between 6 p.m. and 6 a.m., but it is much reduced for some hours before and after noon.

§ 6.

Recently Dr. S. J. Mauchly,§ a member of Dr. Bauer's staff, claims to have established that "the diurnal variation of the potential gradient over the oceans is primarily due to a 24-hour 'wave,' which progresses approximately according to universal rather than local time" (l.c. p. 80), and further "there appear to be good grounds for concluding that in general the 24-hour wave of the potential gradient progresses approximately according to universal time over the entire surface of the earth."

The regular diurnal variation, it may be explained, can always be analysed into a series of Fourier waves, with periods of 24, 12, 8, &c., hours. For the land, Dr. Mauchly's conclusion is limited apparently to the 24-hour wave. At some stations this is the principal wave; at others, e.g., Kew, it is not.

Dr. Mauchly's conclusions, if confirmed, would obviously be of great importance, as the greater part of the earth's surface is oceanic. The subject has been discussed

* Phil. Mag., Vol. 32, p. 282 (1916).
† Meteorologische Zeitschrift, Vol. 47, p. 75.
‡ Klimatologie im Dienste der Medizin, p. 19 (1920).
§ Terrestrial Magnetism, Vol. 28, p. 61.

TABLE II.—*Diurnal Variation of Electrical Elements (Percentages of Mean).*

Hour.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24
Potsdam, year, P	94	90	89	84	85	90	100	112	110	110	102	94	91	88	89	92	95	98	106	115	120	119	113	108
,, ,, λ	106	111	113	113	113	109	101	91	87	87	93	97	99	100	101	100	101	101	99	96	95	94	95	95
,, ,, i	101	101	101	97	97	101	101	101	101	97	93	93	89	89	89	89	93	97	105	114	118	114	109	109
Davos, February, λ_1	144	157	171	182	178	146	100	73	60	45	35	34	43	56	70	83	88	93	90	94	101	106	118	142
,, June, λ_1	140	155	161	169	156	109	68	56	64	70	69	72	74	79	84	90	94	90	86	89	95	104	109	117
Kew, summer, P							112	120	117	109	102	100	84	75	79	78	85	103	111	123				
,, ,, λ							80	78	84	99	96	99	114	119	119	115	113	107	92	86				
,, ,, i							89	96	98	112	101	101	97	93	93	105	102	107	99	107				
Eskdalemuir, August, P				120	158	163	162	115	86	72	72	72	67	62	62	62	62	77	134	153				
,, ,, λ				77	68	72	81	79	118	141	123	120	129	127	107	108	110	83	83	74				
,, ,, i				92	95	110	122	97	111	124	112	101	103	96	99	71	78	72	117	97				

in recent Papers by Prof. G. Angenheister* and Mr. F. J. W. Whipple.† Both refer to the support which Dr. Mauchly's conclusions seem to derive from potential gradient observations in high latitudes. One objection, which had occurred also to myself, is that at Samoa observatory—of which Angenheister was long director —the diurnal variation of potential gradient near the ground appears to be of the ordinary double oscillation type at land stations; whereas in a small island in the Pacific one would *a priori* have expected oceanic conditions to prevail. Angenheister points out, however, that this double oscillation at Samoa seems confined to air very near the ground. At 8 metres the 24-hour wave prevails, but its phase does not accord with that of Mauchly's universal wave. Another circumstance which should be mentioned is that when the earlier portion of the material at Dr. Mauchly's disposal was discussed by Dr. Bauer and Prof. Swann‡ in 1917, the conclusion reached was that the diurnal variation at sea was similar to that at Potsdam and Kew. According to Dr. Mauchly (l.c., p. 61) "the difference between the conclusions of 1917 and those of 1921 is due chiefly to a difference in the method of reduction." It is obvious, of course, that the diurnal variation at a particular spot might consist of two parts, one purely local, the other universal with its maximum phase answering to a fixed hour G.M.T. If results were obtained from a number of stations, set out at equal intervals along a parallel of latitude, and if the part of the diurnal variation dependent on local time had everywhere the same amplitude, then if the hourly values at all the stations were set out according to Greenwich time and combined, there would result a diurnal variation corresponding to the part depending on universal time.

It is this universal part of the diurnal variation which Dr. Mauchly has apparently set out to isolate in the case of the ocean observations. A number of points are obviously involved, on which it would seem difficult if not impossible to form a judgment without a personal study of the material.

As a circumstance favourable to Dr. Mauchly's conclusions, Prof. Angenheister§ refers to a result which I reached when discussing the magnetic observations taken in 1911 and 1912 in the Antarctic at Cape Evans.|| It was then pointed out that the hours of maximum disturbance in the Antarctic appeared fairly similar to those at Eskdalemuir, when both were reckoned in Greenwich time, though local time at the two places differed by some 11 hours. This suggested the possibility that magnetic disturbance all over the earth might proceed according to universal time. If this should prove to be the case, it would, of course, reduce the mistrust with which most people welcome what seems to be an unique phenomenon.

It is perhaps only proper to remark that since discussing the Cape Evans data, I have studied the results obtained by the Australian Antarctic expedition in 1912-13 at Cape Denison. Treating the Cape Denison data in the same way as those at Cape Evans, I again obtain a conspicuous maximum of disturbance in what is the night at Greenwich, but the day at Cape Denison. This maximum, however, seems to be several hours later at Cape Denison—which is to the west of Cape Evans—than it is at Cape Evans.

* Ges. d. Wiss. Nachrichten, Gottingen. Math.-Phys., Klasse, Heft 2, p. 81 (1924).

† Meteorological Magazine, Vol. 59, p. 201.

‡ Department of Terrestrial Magnetism, Carnegie Institution. Researches, Vol. 3, p. 422 (1917).

§ loc. cit., p. 98.

|| British Antarctic Expedition, 1910-1913, Terrestrial Magnetism, p. 144.

§ 7.

To render clear the position as to longer period variations, a brief reference is necessary to terrestrial magnetism. As is well known, the amplitude of the regular diurnal magnetic changes shows a large variation from year to year, at least approximately parallel to the variation in sunspot areas or frequencies. There is also, at least in high latitudes, an increase in the amplitude of the regular diurnal changes on days which are magnetically disturbed. The only reasonable explanation hitherto proposed is an increase in the electrical currents in the upper atmosphere, to which the regular diurnal changes are now generally ascribed. The increased current amplitude is most naturally explained by increased ionization and conductivity. As severe magnetic storms are usually accompanied by Aurora, it is reasonable to suppose that the height of the Aurora indicates roughly the level of the highly ionized atmosphere. Some observers have claimed that they saw Aurora below the summits of mountains, and other phenomena consistent with this view have been described ; but it has received no support from the recent exact measurements of Auroral heights made in Norway by Prof. Carl Störmer* and others. According to these the lower edge of Aurora is usually at a height of from 90 to 100 km. Thus, while magnetic phenomena support the existence of the sunspot period in atmospheric conductivity at heights of 90 km. and upwards, they seem to throw no direct light on its existence in atmospheric electricity in the lower atmosphere. Obviously, however, it would not be at all surprising if it did present itself there, and Dr. L. A. Bauer† claims that it is present and far from negligible in the data published at Tortosa for potential gradient. Kew‡ data are not inconsistent with the existence of a *small* 11-year variation, but they do not suggest any such considerable effect as Dr. Bauer described at Tortosa. There are, unfortunately, few stations having homogeneous data for even one sunspot cycle, so the question is unlikely to be settled definitely for some time to come.

§ 8.

There are a number of possible sources§ for the ionization of the atmosphere, including radioactive emanation in the air itself, γ-rays from the radium and thorium in the soil, photo-electric effects at the ground, ultra-violet light and penetrating radiation. Over the oceans and lakes ionization may be produced by the breaking of waves.

Direct radio-active effect from the soil will naturally be effective mainly if not exclusively in the lowest layers of the atmosphere ; while the effects of ultra-violet light would naturally be much greatest in the highest layers. The radio-activity of the soil and the radio-active emanation in the air seem alone quite adequate to account for the ionization usually measured near the ground. Experiments with closed vessels have shown the existence of a penetrating radiation. The difference between the results obtained over land and water show that on land a substantial part of this comes from the ground. As we ascend, the penetrating radiation,

* Terrestrial Magnetism, Vol. 21, p. 157, and numerous other Papers and memoirs.
† Terrestrial Magnetism, Vol. 27, p. 1.
‡ Proc. Phys. Soc., Vol. 35, p. 129.
§ c.f. Chauveau, Électricité Atmosphérique, Troisieme Fascicule, Chap. 6.

according to the observations of Hess* and W. Kolhorster,† diminishes to a minimum at a height of from 500 to 700 metres, and then increases again. At 2,000 metres it is greater than at ground level and it increases notably at higher levels. Observations made recently by R. A. Millikan‡ in America so far agree with Kolhorster's, but they make the increase with height much less. It has been suggested that part of the penetrating radiation may come from sources external to the atmosphere, but this view does not seem supported by Millikan's observations.

* Physikalische Zeitschrift, Vol. 12, p. 998, &c.

† Physikalische Zeitschrift, Vol. 14, pp. 1066 and 1153, and Terrestrial Magnetism, Vol. 20, p. 82.

‡ c.f. C. V. Ockenden, Meteorological Magazine, Vol. 59, p. 220.

GEOPHYSICAL INFLUENCES ON THE TRANSMISSION OF WIRELESS WAVES.

By Prof. E. V. Appleton, *M.A., D.Sc.*

ABSTRACT.

The facts of short and long-distance transmission for long and short waves are summarised and the examples of variable signals attributed to the varying phase and amplitude of rays ionically refracted by the upper atmosphere. The diurnal variation of such signals is described, and the strong night signals attributed to a lower atmosphere devoid of excessive ionization. Explanations of the difference between the magnitudes of directional errors for overland and over-sea transmission and of the marked success of nocturnal short-wave transmission are put forward.

The salient points of the evidence relating to the seasonal variations of signal strength and to the influence of the configuration and inhomogeneity of the earth's surface are also summarised.

The facts of wireless telegraphy, as set forth show that the atmosphere exerts a variable but usually favourable influence on wave propagation. This influence may be interpreted in terms of ionic refraction (Eccles) which, to prevent excessive dissipation, must take place at levels sufficiently high to make the mean free paths of the effective carriers large (Larmor).

THE various facts concerning the role of the atmosphere in Wireless Telegraphy have resulted from the comparison of the received signal intensities with the theoretical values worked out on the assumption of a perfectly conducting earth and a perfectly insulating atmosphere. It was very soon found that marked discrepancies between observed and calculated values were evident, especially when the transmission took place over large distances, which compelled a closer scrutiny of the physical assumptions underlying the mathematical investigations. The laws of electrodynamics are certainly valid for such low frequencies so that all recent work has been concerned with linking up the discrepancies with the finite resistance of the earth and the ionization in the atmosphere.

In approaching the somewhat complicated facts of signal transmission it is of great assistance to bear in mind certain broadly-defined distinctions. In the first place we must be careful to distinguish between transmission over distances small enough for the earth to be considered plane from transmission over larger distances where the curvature of the earth has to be taken into account and diffraction is all-important. Also we must recognize that the results for short waves (e.g., of the order of 300 metres) are usually quite different from the results obtained with long wave-lengths (e.g., of the order of 10,000 metres). Lastly, we must recognize that the results for day and night conditions are often very different, especially in the case of short waves.

Bearing these differences in mind I have dealt with the subject under two headings, (I) short distance transmission and (II) long distance transmission, and the results with long and short waves during day and night conditions are discussed under these headings. Other points which seem to require special mention are dealt with under headings (III) to (VI).

I. Short-Distance Transmission.

If an alternating current of maximum amplitude I_0 and angular frequency ω passes through an element of length ds, the electric intensity E of the radiation field at a distant point in a plane at right angles to the element is given by

$$E = \frac{I_0 \omega ds}{rV},$$

where r is the distance of the point from the element and V is the velocity of electromagnetic radiation in free space. If this formula is applied to the radiation field of a wireless aerial the value of ds to be inserted (known as the effective height) is the value which fits the equation when E is measured at a point sufficiently far from the origin to make the induction field negligible and yet sufficiently near for there to be no marked absorption of radiation. According to this formula the field intensity should vary inversely as the distance. Duddell and Taylor* nearly twenty years ago carried out the first accurate intensity measurements with a view to testing this relation for short waves over short distances. Over sea-water the inverse-distance relation was found to hold accurately up to 96 km. On the other hand, discrepances were found for overland transmission at distances as small as 1,800 metres. The signal strengths were less than the calculated values due to the absorption of the badly conducting ground. That the departure of the law of decay from the simple inverse distance law is due to the absorbing action of the ground is evidenced by the fact that signals can be obtained with a horizontal antenna pointing in the direction of propagation of the waves. The correlation of such a horizontal component of electric force with ground absorption was indicated by Epstein† and follows directly from an application of Poynting's theorem.

With longer waves the attenuation is by no means as marked over land. For example, Austin‡ compared transmission over 83 km. for wave-lengths of 1,000 and 3,750 metres. In the former case the electric force was less than one-fifth of its calculated value while in the latter case the two agreed within the limits of experimental error. All other observations seem to show that the absorption is only marked for short waves, and that the longer the wave-length the more nearly is the inverse distance law of electric force attenuation obeyed.

The effect of ground absorption for short waves over short distances has been very thoroughly studied recently by Bown and Gillett§ who studied the space variation of electric force round two American broadcasting stations. Their results are embodied in two most instructive diagrams given in their paper, one of which shows, as a graph, the relation between the decay of electric force and the topography of the district, while the other exhibits the field strength contours superimposed on an aerial photograph of the district. These diagrams illustrate in a most marked manner the difference in attenuation over land and over water. This difference may be expressed quantitatively, for, if we assume with Austin that the absorption may be allowed for by multiplying the above expression for the electric force by $e^{-\frac{ar}{\sqrt{\lambda}}}$, where λ is the wave-length, the value of a for dry and sandy ground is 0·28, while for over water transmission it is only 0·0025.

The curves further illustrate the marked shielding influence of mountains, first noticed by Sir Henry Jackson,|| at points on the immediately far side of the

* Journ. I.E.E., Vol. 35, p. 321 (1905).

† Jahrbuch der drahtlosen Telegraphie und Telephonie, Vol. 4, p. 176 (1910).

‡ Bulletin of Bureau of Standards, Vol. 7 (1911).

§ Proc. I.R.E., Jan. 16 (1924), Figs. 3 and 7 in particular.

|| Proc. Roy. Soc., May 15 (1902).

obstacle. As the obstacle was of the same order of dimensions as the wave-length (469 metres) the shielding effect disappeared at greater distances.

The above observations were made during the daylight hours when steady and consistent values can usually be obtained. At night there is a marked difference in that for transmission distances over 50 miles the intensity is variable. These variations in general are more marked the shorter the wave-length, though there is a certain amount of evidence to show that at very short wave-lengths (e.g., 50-100 metres) the variations are less than on higher wave-lengths (e.g., 200-500 metres).

A good deal of information relating to night-time variation of short wave-signal intensity has been published from time to time, mainly by wireless amateurs, but as the results are expressed in terms of telephone audibilities, the measurements are of little scientific value. The only accurate measurements of short-wave intensity variation that I am aware of are those carried out by Pickard in America, and by wireless students* in Cambridge. In these experiments a galvanometer has been used as the recording instrument. The results show that although there is very little variation of intensity during the day, most marked variations occur beginning round about sunset. The effect is very marked at distances of 100 miles, but even at Cambridge, which is only 55 miles from London, the night variations of the waves from the London Broadcasting Station can be detected and measured. The relative magnitude of the changes depends on the relative magnitudes of the ground or direct ray, and the reflected or indirect ray from the upper atmosphere. During the day the indirect ray due to its refraction in the lower layers of the atmosphere is very weak, and the signal is to be attributed mainly to the direct ray. Taking the Cambridge measurements we find that with a receiving set of a certain sensitivity the signal from London is large compared with that from Bournemouth, which is barely measurable. After sunset the variations of the London signal are relatively small, the daytime mean value being practically maintained. On the other hand the Bournemouth signals increase and decrease by relatively large amplitudes, the mean night value being much bigger than the mean day value. It seems difficult to account for these results in any other way than by assuming that the intensity of the indirect ray at night in the case of London is small compared with the direct ray, and that the night effect is to be attributed to interference between the two rays, the varying phase and amplitude of the indirect ray being the determining factors. In the case of Bournemouth signals, however, the indirect ray is much stronger than the direct one, and so the large observed changes are mainly due to the intensity fluctuations of the former.

For distances sufficiently large for the effect of the reflected or indirect ray to be appreciable, marked variations of bearing as measured by coil direction finders are noticed. These variations were first described by Hoyt-Taylor and Eckersley, and were considered by them to be an effect of the upper atmosphere. If we attribute these errors to the interference of the two rays and assume that the reflected one is the variable one, we should expect the errors to be noticeable at shorter distances over land, where the direct ray is strongly attenuated, than over sea, where little absorption takes place. This is actually found to be the case. Directional errors are noticed for transmission over land at distances as small as 30 miles, while over sea errors are rarely significant until the path is 80 miles.

* In particular I should like to mention Mr. M. Barnett and Mr. F. G. G. Davy.

II. Long-Distance Transmission.

Here there is a discrepancy between the theoretical and experimental values for both long and short waves. These differences are found not to be attributable to the finite conductivity of the earth, and the discrepancy is usually put down to the ionization in the atmosphere.

The case of short waves will be considered first. Here the most accurate experimental results are those obtained by the engineers of the Western Electric Company* who, in connection with tests preparatory to the inauguration of a Trans-Atlantic telephone service, investigated the transmission of short waves over sea and over distances for which there is an appreciable curvature of the earth's surface. During the day the values of signal intensity for distances up to 1,100 miles were found to be closely in accordance with the values given by the empirical formula of Austin, which is simply the ordinary radiation formula for a flat earth multiplied by an attenuation factor of the form $e^{-\frac{ar}{\sqrt{\lambda}}}$, where a is 0·0015, when r and λ are in kilometres.

At night much higher values of signal intensity were obtained which agree approximately with the theoretical values if the attenuation factor is neglected. This has led some American engineers to say that the old difficulty of accounting for the large observed intensities for large distances does not exist, but that the atmosphere at night can be regarded as insulating, and a simple inverse distance law is to be expected to hold. Such an interpretation is, of course, in opposition to the results of the mathematicians who say that diffraction alone, especially with short waves, cannot possibly account for the observed intensities. Most probably the explanation is that the reflection of the main indirect ray is almost complete and as its linear path for long-distance transmission is not so different from the direct ray path, the inverse distance law holds approximately.

With longer waves the Austin formula gives fairly accurate prediction, but here the difference between day and night intensities are by no means as marked.

Fuller† in 1914 experimented between Honolulu and San Francisco, over a distance of 3,880 kilometres, the transmission being almost all over sea. The length of wave was increased in steps from 3,000 to 11,800 metres. During the day the received signal increased steadily with increase of wave-length, but during the night the intensity wave-length curve showed maxima and minima. We may interpret this as indicating that during the day the indirect rays are feeble, so that increase of wave-length in increasing the effect of pure diffraction brings about an increase of signal strength, while, at night, when the direct and indirect rays are of the same order of magnitude, interference bands, which alter with the wave-length of the radiation, are produced.

For long-distance transmission the day-time signals are usually much larger than those to be expected from a pure diffraction theory, so that favourable atmospheric influences are indicated even in the day-time. It may further be mentioned that this influence is not always still more favourable at night in the case of very long waves, for the mean day and night signals for wave-lengths of the order of

* Proc. I.R.E., June (1923).
† Electrician, May 7 (1914).

10,000 metres are not very different. The marked difference between day and night signals is to be found only with the shorter waves.

If we are to attribute the excess of signal strength over the theoretical value predicted by diffraction theory to the effect of an ionized layer, the difficulty has been raised that the sun's radiation should produce such a layer in the day time and we ought to expect the effect of the ionized layer to be most marked during the day. The answer to this difficulty seems to be as follows: To produce sufficient refraction to bring an inclined ray horizontal we require a certain increase of phase-velocity at some place midway between the two stations. Due to atmospheric ionization caused by the sun's radiation, this particular increase of phase-velocity is reached during the day at levels at which the pressure is sufficient to make the collisional "friction" experienced by the ions strongly dissipative, and the ray is absorbed. At night, when the atmosphere is cleared, this particular increase of phase-velocity is reached at higher levels, in which the ray can be turned but not absorbed. In the latter case, as Larmor has recently shown, it is not necessary to assume large ionic contentrations. In such a case it is correct to speak about a *conducting* layer, since there is less "friction" there than at the lower levels, but the term *ionized* layer may not always apply, for in some cases it does not seem necessary to consider the effective layer as more ionized than any other layer in the atmosphere.

III. Diurnal Variation of Signal Strength.

Various facts concerning this variation have been mentioned in the general sections relating to long and short waves. When the distance of transmission is not large the direct ray is the principal factor in reception, and practically no variations are noticed. For greater distances the marked but sometimes variable increase of signal strength is the most important phenomenon, especially with short waves. For transmission in an east and west direction curious changes are noticed during the periods when a sunset or sunrise band is between the transmitting and receiving stations. Signals are then abnormally low. The sunset band seems a bigger obstacle to electric wave transmission than the sunrise band, for the received signals are almost consistently lower during the period between the two local sunsets than between the two local sunrises. During the time when the sunrise band is between the two stations a temporary maximum is often noticed, so that two sunrise minima are obtained. One marked minimum seems to be the effect of the sunset period.

The effect of a solar eclipse has been in almost all cases a partial or complete return to night-time conditions, an increase of signal strength being the rule.

IV. Seasonal Variation of Signal Strength.

Very few observations on the nature of this variation have been made, but the general result of the available evidence is that for long-distance transmission the signals are stronger during the winter than during the summer, but that the relative amount of winter increase seems to vary from year to year. It is usually found that the annual variation is more marked for night transmission than for transmission during the daylight hours.

V. Effect of Direction of Transmission.

Some observers have noted that it is easier to signal in a north and south direction than in a direction at right angles. Whether this is due to the influence of the earth's magnetic field or is merely due to the absence of sunrise and sunset bands in the former case remains to be decided.

It seems fairly well established that directional variations as indicated by a loop receiver are less troublesome for north-south transmissions than for the direction at right angles.

VI. Influence of Configuration and Inhomogeneity of the Earth's Surface.

The shielding influence of obstacles (e.g., mountains and trees) was first described in detail by Sir Henry Jackson; such effects are, as we should expect, the more marked the shorter the wave-length. Quantitative values of signal strength relating to the shielding effect of mountains have, as previously mentioned, been given by Bown and Gillett.

The different conductivities and dielectric constants of sea-water and land lead to different velocities of propagation of waves along them, and Eckersley* has shown how this accounts for the refraction at coast lines. The velocity of the waves over sea-water is greater than over land, resulting in an effective refractive index of the order of 1·02. Similar effects were found by Bown and Gillett in America. The importance of refraction of this kind diminishes when the length of the wave increases.

Appendix I.

In the above account of signal variations I have, for conciseness, used the word "reflection" with reference to the ray returned from the upper atmosphere. It is, of course, unlikely that there is a boundary between a non-conducting layer and a conducting one, the transitional element of which is, in dimensions, comparable with a wave-length of the radiation. The ray must be returned by a kind of ionic refraction due to a phase-velocity increased by the presence of free ions of large mobility. The theory of metallic media here applies, as suggested first by Eccles,† but it has been shown recently by Larmor‡ that the kind of conditions contemplated by Eccles involve so much absorption that the effect on the received wave would be almost negligible. The way out of the difficulty seems to be to assume, with Larmor, that the part of the atmosphere which is effective is that in which the electrons or ions experience very little collisional "friction." The question remains to be decided as to whether electrons or molecular ions are effective, and the question may be decided, I think, by considering the action of the earth's magnetic field in connection with wave transmission. If we consider transmission along the earth's magnetic field H_0 in a medium containing electrons or ions we find that the phase velocity v becomes (in Heaviside units)

$$v = \frac{c}{\left(1 - \dfrac{Ne^2}{\omega^2 m \pm \dfrac{eH_0\omega}{c}}\right)^{\frac{1}{2}}}$$

* Radio Review, Vol. I, June (1920).
† Proc. Roy. Soc., Vol. 87 (1912).
‡ Nature, November 1 (1924).

where c is the velocity of light, N the number of charged particles of mass m and charge e per c.c., and ω the angular frequency of the radiation. We thus see that a linearly polarized ray in such a case would be split up into two circularly polarized rays travelling with different velocity, and the resultant plane of polarization would be rotated in the usual way. Such a rotation may be expected to have an effect on the observed direction of the waves as indicated by a direction-finder. But the chief interest of the formula seems to be that for a wave-length of about 580 metres and a magnetic field H_0 of the order of 0·18 gauss, the terms $\omega^2 m$ and $\frac{eH_0\omega}{c}$ are of the same order of magnitude for electrons, in which case a kind of resonance would take place for one of these rays. This frequency corresponds to the frequency with which the free electrons circle round the lines of magnetic force, due to the velocity of thermal agitation. Thus peculiar things might be expected to happen for wave-lengths round about this value if the carriers are electrons. On the other hand if the carriers are molecular ions, this critical wave-length would be of the order of 10^6 metres, and so would be completely outside the wireless spectrum, except so far as atmospherics are concerned.

On general grounds we should expect the lack of absorption due to the large electronic or ionic free paths to be more marked the higher the frequency, for at high frequencies the electron or ion would make many oscillations between the time of two collisions. Possibly this is the explanation of the remarkable success of amateur transmissions with low power and short waves at night.*

Appendix II.

I have consulted one or two of the amateur experimenters who have met with such marked success in sending signals over large distances with low power and short wave-lengths. I find that the exact value of the series capacity in the aerial seems to be all-important, in which case we may infer that the mode of excitation of the aerial has a good deal to do with their efficient transmission. It seems most probable that the aerial must be excited at a frequency higher than its fundamental, in which case, for resonance periodicities, as shown by Van der Pol,† the direction in which maximum energy is radiated is not along the ground, but in a direction inclined to it. We thus arrive at the somewhat remarkable conclusion that to prevent marked attenuation we need only keep the wireless ray off the ground; in other words, for efficient transmission at night, we must take special steps to send the maximum amount of energy into the upper atmosphere.

* If this suggestion is correct we might expect that the reduction of the wave-length to very low values would make daylight transmission of small attenuation possible in the lower layers of the atmosphere which are ionized by solar radiation; for a tenfold increase of frequency and a tenfold reduction in the time between two ionic collisions (due to a reduction in the height of the effective atmospheric layer) would leave the co-efficient of absorption unaltered.

† Proc. Phys. Soc., June 15 (1917). In this Paper Van de Pol actually suggests that a loaded antenna of this kind might be used for detecting the ionic refraction in the upper atmosphere.

ATMOSPHERICS.

By R. A. Watson Watt.

Abstract.

The Paper presents a brief review of data, as to the nature and origin of atmospherics, obtained in the course of a fundamental investigation of atmospherics undertaken by the Radio Research Board. The determination of the wave form of atmospherics by cathode ray oscillograph is referred to, and typical constants of peak field strength and duration are quoted. The results of an examination of the meteorological conditions prevailing at apparent sources of atmospherics, as located by direction finding, are tabulated, and show that even with the limited data available relations with thunderstorm phenomena, precipitation, or barometric minima have been traced, in 87 per cent. of 490 cases, distributed over Europe, the Mediterranean, and North Africa. The tracing of a "cold front" for 40 hours and 2,000 k. across Western Europe, by directional recording of atmospherics, is reported. Reference is made to the fine structure of atmospherics in relation to their interferent properties. The present state of the investigation is summarised.

I. Introduction.

THE inclusion in a discussion on "Ionization in the Atmosphere" of these notes on naturally occurring electromagnetic waves of relatively low frequency may, like the short title "Atmospherics" now generally applied to such waves, be suspected of begging the question of whether atmospherics are in fact atmospheric. But with the reservation that in the present state of our knowledge we cannot determine whether all atmospherics are products of ionization in the terrestrial atmosphere, it may be asserted that a very large proportion of atmospherics are such products, that there is no substantial evidence of any other origin for atmospherics, and that the known facts as to atmospheric processes are compatible with the hypothesis that the known supply of atmospherics, great as it is, results from ionization and convection in the atmosphere.

The atmospheric, in common with the most prominent of the meteorological elements, has a dual aspect, first as a geophysical phenomenon, and second as an interfering agent in human activities, and as in the case of its meteorological fellows its interferent effects have compelled attention to its geophysical origins. It is, accordingly, the purpose of this note to review very briefly some of the evidence which has been obtained in recent years as to the nature and origin of atmospherics, and to refer to some new data explaining their formidable powers of interference in radiotelegraphy.

II. Wave Form of Atmospherics.

Reversing the chronological order in which the data were obtained, a summary of recent determinations of the nature of atmospherics may be given. In a Paper by Prof. Appleton and the writer* will be found a description of the method by which the temporal variation of electric field constituting an atmospheric, and which may be briefly called the "wave-form" of the atmospheric, has been delineated. The method consisted in coupling a simple aerial circuit of known constants, by way of a coupling condenser, to a sensibly distortionless triode amplifier,

* Watt and Appleton, "The Nature of Atmospherics—I," Proc. Roy. Soc., A, 103, 84 (1923).

whose output was applied to a sensitive cathode ray oscillograph. The screen image produced by the simultaneous application of the amplifier output voltage and of a locally generated and controlled voltage, providing a time base, was delineated by hand.

Since the publication of that Paper the collaboration has continued, with the assistance of Mr. J. F. Herd, and has been supplemented, through the courtesy of the Admiralty, the War Office, the Air Ministry, the Physical Department of Egypt, and the Sudan Government, by the delineation of typical wave-forms of atmospherics in the Middle East. As a result of this work, there are at present in process of reduction, or awaiting reduction, more than fifty thousand drawings of atmospherics observed in south-east England, the Mediterranean, Red Sea and Indian Ocean, in the vicinity of Cairo, and at Khartoum.

It is, however, sufficient for the purpose of the present discussion to outline the main characteristics of atmospherics observed in winter in south-east England. From the first six hundred samples analysed it was found* that the average atmospheric produced a maximum field change of the order of one-eighth of a volt per metre, and that the disturbance from the level field strength persisted for periods of the order of one or two thousandths of a second. Two main types occurred almost

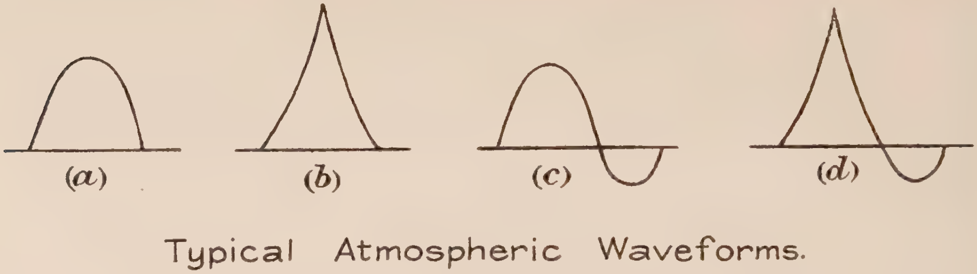

Fig. 1.

equally frequently: in one the disturbance consisted of a departure to one side of the level field, followed by a departure in the opposite side—i.e., a quasi-periodic disturbance consisting of one complete oscillation, whose mean duration was of the order of 2,000 micro-seconds, and in which the maximum electromotive force produced was most frequently directed down the aerial. The two main sub-types of this class are represented in (*c*) and (*d*) of Fig. 1. In one instance four complete oscillations were observed in a disturbance of this type. The second main type consisted of an aperiodic disturbance, most frequently with a duration of some 1,250 micro-seconds, and with a peak field strength change of the same order as in the quasi-periodic type. The main sub-types of aperiodic disturbance are represented in (*a*) and (*b*) of Fig. 1.

The wide departure of these forms from the simple exponential type which had customarily been assumed in theoretical work is of considerable interest. These early observations were made with a sinusoidal time-base, with which it was difficult to discriminate the order of temporal incidence of the various portions of an atmospheric, and all subsequent observations have been made with a linear unidirectional

* Watt and Appleton, loc. cit.

time base developed for this work by Appleton, Herd and Watt. This discriminating time base makes possible a fuller classification of sub-types of atmospheric; the data now being prepared for publication are being classified according to the order of incidence of steep and gentle slopes, and of the varieties of half-cycle. No profound modification of the fundamental values discussed is likely to arise from the later English observations.

Many of the fundamental wave-forms observed were found to have a fine structure, in which the elements were comparable in duration with the lower commercial radiotelegraphic frequencies; attention was drawn to this fact in the Paper under reference, and a further note on this structure will be found later in the present Paper.

III. Extreme Values.

The extremes of field-strength, duration and numerical frequency experienced in the course of the observations are, naturally, very diverse. The number of atmospherics per unit time passing a selected peak field strength increases very steeply as the selected field strength decreases. At the upper end of the scale of radiation fields definitely measured stands an atmospheric observed at Khartoum, which produced a voltage of 250 in the aerial, corresponding to 4·2 volts per metre. As to the lower limit of duration one can say little more than that the present limits of resolving power of the combination of cathode-ray oscillograph and observer reveal many apparently isolated peaks of the order of 100 micro-seconds. The average lightning flash would appear to produce a series of detached complex discharges, in which some three to six discharges, of duration about 0·003 sec., are separated by quiet intervals of 0·002 sec. to 0·003 sec., giving a total disturbed period of the order of 0·01 sec. to 0·03 sec. In one instance, however, near Salonica, in June, 1924, the writer has observed many lightning discharges in which there was no sensible interruption of the visible illumination, and sensible continuity of the audible disturbance in a radio-receiver, over periods of four and five seconds.

Simultaneous measurements have also been made, abroad and in south-east England, of the number of atmospherics per unit time whose peaks exceeded specified limits, but the results await reduction.

In relation to the adequacy of the processes of atmospheric ionization to account for the whole supply of atmospherics, it is very necessary that a reliable estimate of the world supply of lightning may be made and published. As a very slight contribution to the estimate, the writer may record that in his brief experience of the Tropics he observed, on at least three occasions (one in November on the African coast of the Red Sea, two in April at Khartoum), groups of storms in which flashes occurred at a mean rate of one per second throughout a period of three hours. Doubtless higher flash frequencies are experienced in the true thunderstorm seasons in those latitudes.

IV. The Atmospheric as a Meteorological Phenomenon.

Turning to the atmospheric as a meteorological phenomenon, no more than a passing reference is now required to the early work on the meteorological relations of atmospherics which has been summarised in a Paper by Mr. C. J. P. Cave and the writer.*

* Cave and Watt, "Study of Radiotelegraphic Atmospherics in Relation to Meteorology," Q.J. Roy. Met. Soc., 49, 35 (1923).

It is now possible, however, to give revised data as to the relation between apparent sources of atmospherics, on the one hand, and thunderstorms and other meteorological phenomena on the other, which formed the subject of a preliminary note in " Nature."*

In the investigation of this relation the results of observations on the direction of arrival of strong atmospherics at British stations were employed, the apparent source of the predominant atmospherics at certain times being found by the intersection of bearings from three or more stations. These locations of apparent sources were compared with meteorological data for the time and place concerned, much unpublished thunderstorm data having been supplied (since the date of the note in " Nature ") by foreign meteorological Institutes through the courtesy of the Director of the Meteorological Office, Air Ministry. It is to be regretted that time has not yet been found for a really exhaustive study of the non-thunderstorm data, the method adopted has been to determine the place and time of occurrence of the nearest reported thunderstorm, and, in the absence of thunderstorms, to search for records of hail, passing showers, squalls and rain, and to examine the barometric distribution. In order to present a brief summary of the result Table I. has been prepared, showing the number of apparent sources of atmospherics located in various regions between April, 1916, and April, 1918, with the percentage number of such locations which have been identified as associated with

(*a*) Thunderstorm phenomena within 250 kilometres.
(*b*) ,, ,, 1000 ,,
(*c*) Hail showers within some 200 kilometres.
(*d*) " Passing showers " within a similar distance.
(*e*) Squalls.
(*f*) Rainfall.

The allotment of any individual case to one column implies the absence of reported phenomena belonging to earlier columns—otherwise expresed, the cases are alloted to the highest degree of " thunderiness " reported. The inadequacy of the criteria is clear; hail, passing showers and squalls have not yet been limited to meaning the same things for all observers; and all the criteria adopted, save lightning, are merely collateral effects, and not causes, of the phenomenon of electromagnetic radiation. But in the default of a closer study of the physical processes the classification may serve. It is supplemented by a column in which, when no thunderstorm, squall or precipitation phenomena have been traced, the occurrence of barometric minima, troughs of secondary and V-shaped depressions, and marked local irregularities of isobars near the time and place of apparent origin of atmospherics, has been noted. The re-study of the whole of the data on the lines of this column, or preferably with reference to the " cold fronts " and " warm fronts " with which isobaric irregularities are associated, would be interesting. Meanwhile, the fact that a meteorological correlation of sorts has been found in all save 3 per cent. of the British cases, and, with very inadequate data, in 87 per cent. of all cases is

* Watson Watt, " The Origin of Atmospherics," Nature, 110, 680 (1922).

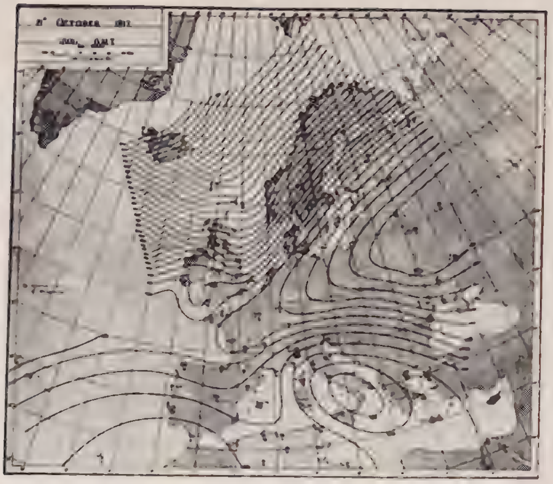

FIG. 2A.

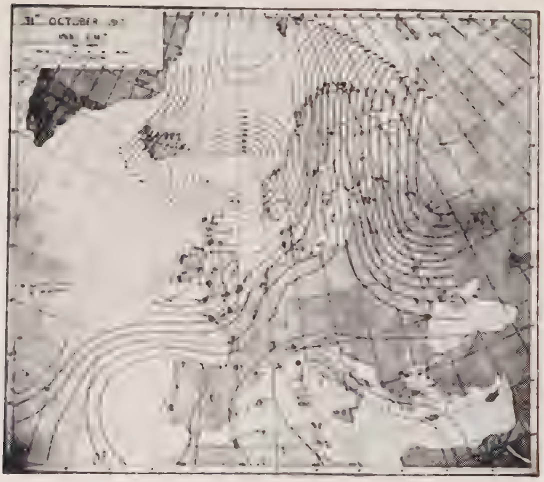

FIG. 2B.

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significant, especially in view of the comparatively large number of locations, averaging two per three days during the period.

TABLE I.—*Meteorological Conditions near Apparent Sources of Atmospherics.*

Region.	No. of locations	Thunderstorms within 250 k. %	Thunderstorms within 1,000 k. %	Hail %	Passing showers %	Squalls %	Rain %	Isobaric minima &c. %	No correlation found. %
England & Wales	58	35	17	5	14	14	7	6	2
Ireland	45	13	13	2	44	16	8	0	4
Scotland	17	0	29	18	35	6	6	0	6
British Isles*	120	22	18	6	28	13	7	3	3
English Channel...	58	31	7	2	20	9	15	7	9
France	146	36	4	2	1	1	27	13	16
Spain	23	13	—	—	—	—	52	13	22
Italy	19	42	—	—	—	—	26	21	11
Holland	7	86	14	—	—	—	—	—	0
S.W. Europe*......	373	30	9	3	13	6	20	9	10
Central and S.E. Europe	12	33	8	—	—	—	—	34	25
Bay of Biscay ...	38	—	11	3	—	—	60	3	24
Iceland and Atlantic	10	—	10	—	—	—	10	50	30
Belgium	7	29	43	—	—	—	—	—	29
Switzerland	6	33	—	—	—	—	—	17	50
Mediterranean and N. Africa	44	2	27	—	—	—	11	46	14
TOTAL	490	25	11	2	10	5	21	13	13

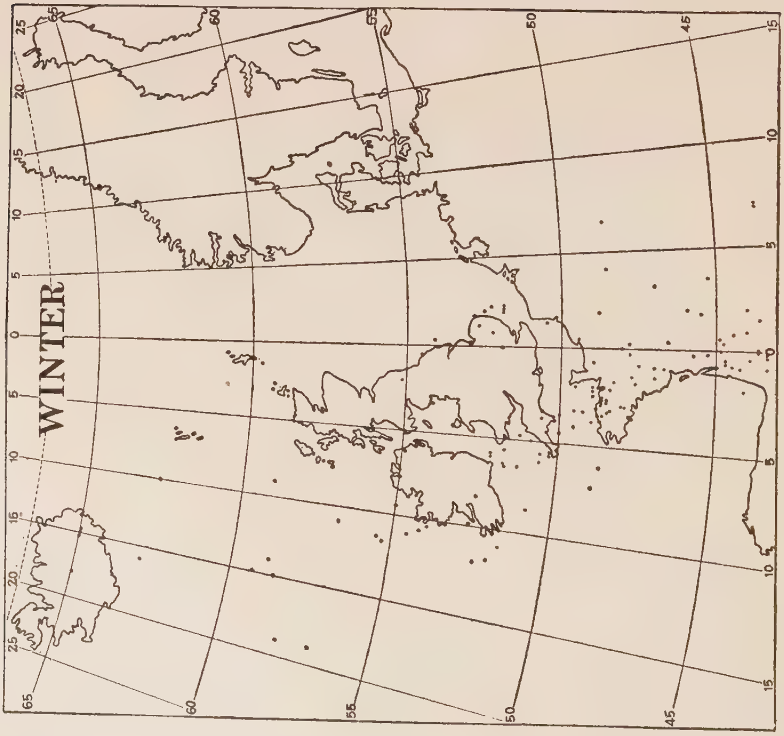
The mean distance between an apparent source and the corresponding "near" reported thunderstorm is approximately 100 kilometres, the corresponding value for "distant" thunderstorms is between 300 and 400 kilometres. Distances over 500 kilometres are rarely used save in regions where data is insufficient—e.g., Italian thunderstorms have had to be used in considering North African sources. It should be noted that the distance between apparent source and reported storm is not to be regarded as a measure of error in location, the storm is merely the nearest reported index of conditions in the vicinity of the source. In 21 cases the location fell within 20 kilometres, and in 44 within 50 kilometres of a reported thunderstorm.

Of special interest are some of the cases in which the apparent source of atmospherics lies in a barometric minimum from which there is a definite report of good weather without thunder or precipitation. The synoptic charts for two such cases are shown in Figs. 2A and 2B.

A further line to be prosecuted in regard to these data, and in relation to thunderstorm data, is the explanation of the general diurnal and seasonal variations in apparent direction of arrival of atmospherics. Fig. 3 shows the distribution of

* These lines represent sums from preceding regions, the three apparently arbitrary groups thus constituted being based on consultation of meteorological data of decreasing adequacy.


SPRING


WINTER

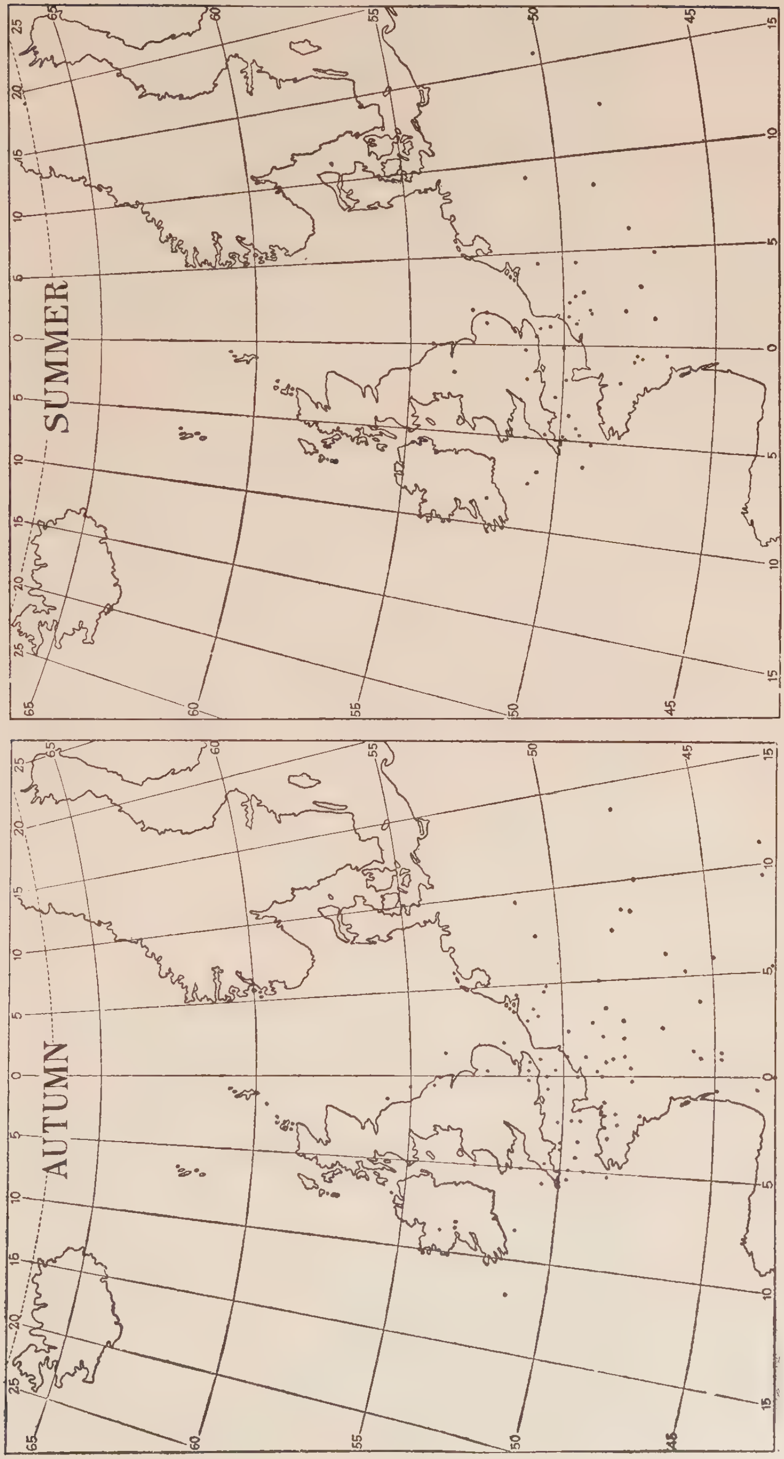


Fig. 3.—Seasonal Distribution of Apparent Sources of Atmospherics.

apparent sources in the four seasons, which may be compared with the published directional data,* and which has in itself some features of meteorological interest.

V. A Cold Front Traced Across Europe.

A recent example of the manner in which the European meteorological situation may dominate the supply of atmospherics in these Islands has been investigated by Mr. J. F. Herd, and by the Meteorological Office.

The Radio Research Board has for some years maintained a directional recorder for atmospherics at their Aldershot Station. In July of the present year a second recorder built at that station was installed at the Geophysical Observatory at Lerwick. After two days' trial run the recorder began routine work at 10 a.m. on July 12, 1924. The meteorological situation at 7 a.m. on that day was as shown on the synoptic chart, Fig. 4A. The well-marked trough then situated over the Atlantic can be followed on the synoptic charts for the 13th and 14th, Figs. 4B and 4C, and more clearly on the chart of isochrones of trough position, Fig. 5, kindly supplied by the Director of the Meteorological Office. At 2 p.m. on the 12th, the directions of arrival of atmospherics at the two stations intersected off the Hebrides. The apparent source of atmospherics was traced hour by hour by similar intersections, the trajectory is plotted on the isochronous chart. It will be seen that the sources of atmospherics are located close to the cold front which lies along the barometric trough, and are in very satisfactory agreement with reports of thunderstorms on the front between 1 p.m. on the 12th at Stornoway, and 1 a.m. on the 14th at Posen.

It is of incidental interest that during the period of six dark hours, shown dotted on the trajectory, no minimum direction of disturbance (from which is inferred the direction of arrival) could be determined from the record for either station—both records ceased simultaneously to be useful directionally, and resumed usefulness simultaneously.

VI. The Fine Structure of Atmospherics.

Reference has already been made to the interferent properties of the atmospheric in radiotelegraphy. It has been remarked† that the relatively smooth fundamental wave forms delineated in the Paper to which reference has already been made‡ are quite inadequate to produce the amount of disturbance which is in fact experienced in radiotelegraphic receivers. This was sufficiently obvious from the beginning, and the reference in the Paper to short period ripples should not have been overlooked as an explanation of the discrepancy. It may, however, be permissible to expand the evidence from more recent observations.

In the first instance it may be remarked that no results were, in the Paper, quoted from measurements on an alternative, and in many respects more attractive, circuit in which the ordinates of the oscillogram are proportional, not to the instantaneous value of the field change constituting the atmospheric, but to the first differential of that quantity, i.e., to the rate of change of field. The reason for this

* Watson Watt, "Directional Observations of Atmospherics—1916-1920," Phil. Mag., 45, 1010 (1923). Watson Watt, "Directional Observations of Atmospheric Disturbances—1920-1921," Proc. Roy. Soc., A., 102, 460 (1922).

† Moullin, 'Atmospherics and their Effects on Wireless Receivers," Jour. I.E.E., 62, 353 (1924). Eckersley, "The Energy of Atmospherics," Electrician, 93, 150 (1924).

‡ Watt and Appleton, loc. cit.

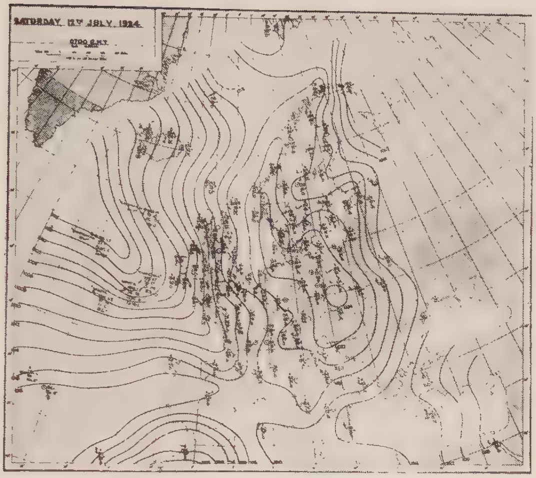


FIG. 4A.

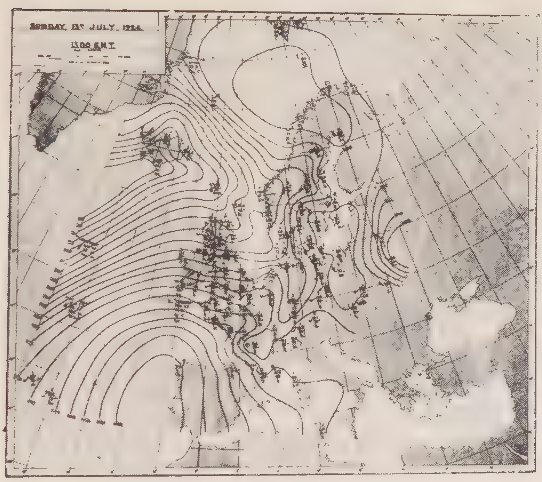


FIG. 4B.

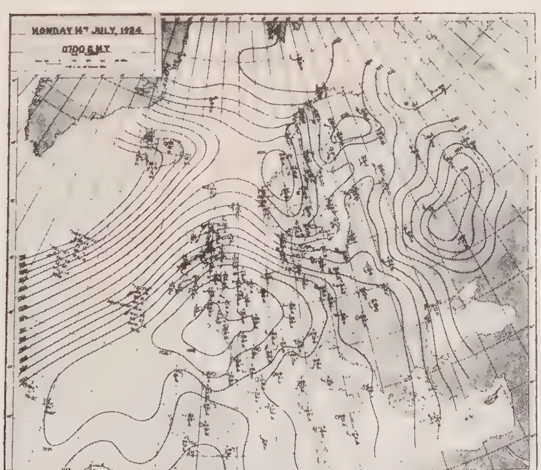


FIG. 4C.

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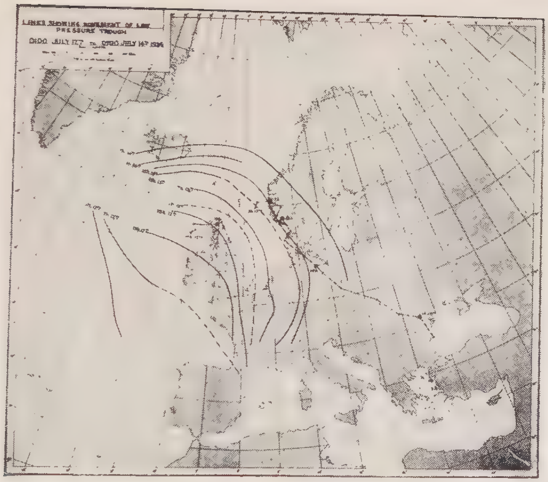

FIG. 5.

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was mainly that the fine structure of many atmospherics, even in the comparative quiet of a British winter, was so well-developed that the oscillogram of rate of change of field was too complex to be delineated or resolved with the apparatus and experience then available.

Experience in the tropics and in sub-tropical regions immediately revealed the fact that this fine structure increased rapidly in relative amplitude with decreasing latitude, and was particularly prominent during the hours of darkness. A fuller investigation of this phenomenon must await the reduction of the wave form data collected, but some preliminary notes on the details of this fine structure, as observed at Khartoum in April, 1924, may be of immediate interest.

In low latitudes, and at night, the ripples rise in importance from 10 per cent. relative amplitude on less than 10 per cent. of all forms observed, as was the case in the first series of wave-form determination, to a relative amplitude varying from 10 per cent. up to 100 per cent. on not less than 70 per cent. of all individual forms examined on a sufficiently open time base.

Figs. 6 to 8 (not reproduced) are untouched photographs of the original drawings of a few typical dark-hour atmospherics from Khartoum, showing, successively, details of wave structure observed on time bases of 250 and 500μs, typical oscillograms of the rate of change of field from lightning within visible range, an oscillogram of the change of field from lightning just beyond visible range, and a random page of observations on a base of 1,000μs.

VII. Conclusion.

In conclusion, the present position of the geophysical study of atmospherics may be summed up as follows. The observational methods available for the study of wave form are, as developed by Appleton, Herd, and Watt, such that detailed examination of components whose duration is of the order of 100μs may be carried out with ease, while observation down to 25μs can be carried out. The necessary instrumental improvements to increase resolving power in this direction to a limit certainly below 10μs are attainable without further experiment, and are being undertaken. The existing apparatus and methods are sufficient to enable a specification to be drawn up, from sampling observations, of the total number per unit time of atmospherics passing specified limits of field strength and rate of change of field, and of the relative distribution of types, in any region. The origin of the prominent atmospherics received in Great Britain has been traced to the European meteorological situation ; improved means have been devised for the prosecution of such studies and are being applied as part of the fundamental study of the nature and origin of atmospherics which is being carried out by the Radio Research Board. Like all geophysical studies this investigation cannot be completed without co-operative effort ; a start has been made within the British Empire, but there is no doubt that the time is close at hand when European collaboration will be essential to progress.

Thanks are due to the Radio Research Board established under the Department of Scientific and Industrial Research, and in particular to its Committee on Atmospherics, for the provision of facilities for the work outlined in this Paper, and for permission to publish the results. Thanks are also due to the bodies and individuals mentioned in the Paper, for facilities, active help, and collaboration.

THE ELECTRIC FIELD OF A THUNDERCLOUD AND SOME OF ITS EFFECTS.

By C. T. R. WILSON, *F.R.S.*

ABSTRACT.

A thundercloud is an electric generator in which the separation of positive and negative charges occurs at a rate corresponding to a current which may amount to some amperes ; the potential difference between its poles may reach values of the order of a million kilovolts. Attention is drawn to three effects of the electric field of thunderclouds or shower clouds (all of which may occur even when there is no thunder). (1) The electric field of the cloud may cause ionization at great heights, the result being continuous or discontinuous discharge between the cloud and the upper atmosphere. (2) Discharge from pointed earthed conductors is likely to constitute an important part of the current between the ground and the base of a thundercloud, and the resulting ionization near the ground may be large. (3) By its accelerating action on particles the electric field of a thundercloud may produce extremely penetrating corpuscular radiation.

INTRODUCTION.

WE may regard a thundercloud as a great electric generator. I shall not attempt to discuss its probable mechanism, whether, for example, it resembles more closely that of a frictional or of an influence machine. The principal theories of its action are those of Simpson and of Elster and Geitel.

Observations on lightning discharges and on the changes in the electric field which are associated with them indicate that the rate of separation of positive and negative charges within a thundercloud frequently corresponds to a current of some amperes, and that potential differences of the order of one million kilovolts are developed.

The charges separated in the thundercloud may re-combine directly by a short-circuiting discharge within the cloud or by continuous or discontinuous discharges through external circuits, one such circuit including the earth and the upper atmosphere.

The changes produced in the electric field by lightning discharges at different distances are most easily interpreted if we suppose that the upper charge of a thundercloud may be either positive or negative, but that it is more frequently positive than negative.

The most conspicuous effect of the electric field of a thundercloud is the production of lightning discharges. I do not propose to discuss the phenomena of ordinary lightning discharges ; I shall confine myself to the consideration of three less obvious effects which may result from the electric field of a thundercloud. These effects are: (1) ionization in the upper atmosphere ; (2) ionization by point discharge from earth-connected conductors ; and (3) the production of penetrating radiation.

I. IONIZATION IN THE UPPER ATMOSPHERE BY THE ELECTRIC FIELD OF A THUNDERCLOUD.

Each of the charges (upper and lower) of the thundercloud forms with its image in the earth a system which has an electric moment $2\Sigma qh=2QH$, where q is the charge at any height h, Q is the whole charge, and H its mean height above the

ground. The electric moment of the thundercloud is the difference between the electric moments of its upper and lower charges. (Where the electric moment of a charge, or of the whole cloud, is spoken of it is to be understood that the effect of the image is included.) At distant points on the earth's surface (not, however, so distant that the curvature of the surface has to be taken into account) the vertical electric force due to a thundercloud of moment M is M/r^3, where r is the horizontal distance of the point from the centre of the thundercloud. The sudden changes in the electric moment of the thundercloud which result when one or both charges are wholly or partially destroyed by lightning discharges may be found by observing the sudden changes in the vertical electric force at the ground at known distances. The mean value of such changes in the electric moment of thunderclouds, or, in other words, the mean value of the electric moment of a lightning discharge, is found to be of the order of 3×10^{16} e.s.u. cm. The average electric moment of a thundercloud on the point of discharge is likely to be greater.

The electric force due to a cloud of electric moment M, at a point vertically above it in the upper atmosphere, may be taken as approximately $2M/r^3$, where r is the height of the point above the ground. While the electric force due to the thundercloud falls off rapidly as r increases, the electric force required to cause sparking (which for a given composition of the air is proportional to its density) falls off still more rapidly. Thus, if the electric moment of a cloud is not too small, there will be a height above which the electric force due to the cloud exceeds the sparking limit.

At a height of 60 km. the density of the air is about $1{\cdot}6 \times 10^{-4}$ of that near the ground, while the composition of the air is not very different, so that the critical value of the field may be taken as about $30{,}000 \times 1{\cdot}6 \times 10^{-4} = 4{\cdot}8$ volts per centimetre. To produce such a field at this height a thundercloud would require to have an electric moment of $1{\cdot}7 \times 10^{18}$ e.s.u. cm.; to produce a field of the critical value at greater heights a smaller electric moment would be sufficient, but the data as to composition and density become uncertain. If we assume the critical field to remain proportional to the pressure, a thundercloud with an electric moment of about 1/10 of the above value (i.e., only a few times the average electric moment of a lightning flash) would produce a field exceeding the critical value at a height of 80 km. Thus, if there were no already existing conducting layer there is little doubt that a thundercloud would itself cause ionisation in the upper atmosphere.

Let us assume that there is already a conducting layer with its lower boundary at, for example, 80 km. Ions are dragged out of this by the field of the cloud, positive if the upper pole of the cloud is negative, negative (probably as free electrons) if it is positive. If the field exceeds a critical value, considerably less for the negative than for the positive ions, the ions acquire sufficient energy to cause ionization by collision. If the field is strong enough to cause ionisation by collision for any considerable distance below the original level of the conducting layer, it is possible that sudden discharge of the upper pole of the thundercloud to the upper atmosphere may occur.

The presence of the conducting layer increases the vertical electric force at points below it. Again, the value taken for the critical field (the sparking value) corresponds according to Townsend's theory to ionization by collision by positive ions; a smaller field is sufficient to give to negative ions the energy necessary for ionization. Thus the estimate given above for the electric moment required to

cause discharge at 60 km. is probably an excessive one, especially when the cloud is of positive polarity.

The vertical electric force at a point at a height of 60 km. may be due, not solely to a thundercloud vertically below it ; all thunderclouds within a considerable area will contribute to it. A cloud 10 km. away from the centre of this area would produce at the given point a vertical force exceeding 9/10 of that which it would produce if it were at the centre. It is possible that the electrical field of a large rain cloud, which would not be regarded as a thundercloud, may be strong enough to cause discharge in the atmosphere above it. An average electric moment of about 5×10^{15} per sq. km. would be more than sufficient to produce discharge at 60 km. if the cloud covered a circular area of 10 km. radius. This value of the electric moment may easily be attained while the electric force within and below the cloud remains far short of the sparking value. The discharges above the cloud would doubtless give rise to atmospherics. If, as has been maintained, atmospherics frequently originate in regions of rain unaccompanied by thunder, they may in such cases be due to discharges of this nature.

II. Ionization by Point Discharge from Earth-connected Conductors.

The electric field at the ground below a thundercloud very often exceeds 10,000 volts per metre, and below the centre of the cloud it probably considerably exceeds this value ; observations made in a properly exposed situation below the centre of a thundercloud are naturally difficult to obtain. Even below rainclouds without thunder potential gradients of several thousand volts per metre are common. Any pointed earth-connected conductor, such as a lightning conductor, projecting to a height in a field of a few thousand volts per metre must give rise to a glow or brush discharge, and a conductor need not end in a sharp point or project to a great height in order that it should begin to act as a discharger. A very simple calculation is sufficient to show that an earth-connected sphere of 1 cm. radius need only be raised to a height of 3 metres in a field of 10,000 volts per metre in order that the electric force at its surface may reach the critical value of 30,000 volts per cm. That considerable currents should be obtained in experiments like that made long ago by Benjamin Franklin with his kite, and that " St. Elmo's fire " should occur in exposed situations, is not therefore surprising.

A considerable part, possibly the greater part, of the current between a thundercloud and the ground must be carried by trees and other natural lightning conductors. It is even more difficult to obtain measurement of this portion of the current (that down a tree, for example) than of the charges carried by rain or by lightning. Useful information bearing on the matter—and particularly on the very fundamental question whether the current between the ground and a thundercloud is on the whole mainly upward or downward—is likely to be obtained by observations with lightning conductors erected for the purpose and connected to instruments for recording the current through them. Observations of the sign and magnitude of the intense potential gradients below thunderclouds will also help towards obtaining some estimate of the currents. I have lately been testing methods for making both types of observations.

Some years ago I made experiments to determine what is the magnitude of the potential gradient which has to be applied over the surface of a field of grass to make the tips of the grass blades come into action as point dischargers. With a

positive potential gradient, i.e., with the grass negatively charged, the discharge from a square metre of the grass-covered ground begins to be measurable when the applied potential gradient reaches about 15,000 volts per metre, and the current increases with great rapidity as the potential gradient is further increased ; it soon reaches values of the order of 1 micro-ampere per sq. metre, i.e., of 1 ampere per sq. kilometre. A somewhat larger potential gradient, about 20,000 volts per metre, is required to start the point discharge when the ground is positively charged, i.e., when the potential gradient is negative, and a larger gradient is required to produce a given current. This difference is in accordance with what is known of positive and negative point discharges.

It is of interest to consider the vertical current due to such discharge from grass-covered ground below a thundercloud under ideally simple conditions. Let us suppose that no rain is falling and that the whole current between the ground and the cloud is carried by ions supplied by point discharge from the grass. To simplify the problem, suppose that we have immediately below the thundercloud a vertical field which is uniform over an area of dimensions great compared with the height of the base of the thundercloud. Let us suppose that this field approaches the sparking limit in intensity, and that it is directed upwards, the base of the cloud being positively charged.

If an electric field of this intensity extended as far as the grass-covered ground, this condition could only last momentarily, since a continuous discharge of negative electricity would at once occur from the grass until the potential gradient at the ground only slightly exceeded that required to maintain point discharge from the grass. If a steady condition is reached there must be a streaming of negative ions upwards from the ground to the positively charged base of the cloud ; the space between the ground and the cloud will then contain a negative volume charge and the field will increase with increasing height above the ground. If, in spite of the upward stream of negative ions the electric force immediately below the cloud is maintained at a high value, this can only mean that the positive charge at the base of the cloud is being replenished (by the source of E.M.F. within the cloud) as fast as it is being neutralised by the upward streaming negative ions. The conditions are not essentially different when the potential gradient is negative, so that the upward streaming ions are positively charged.

If k is the mobility of the upward streaming ions (their velocity under unit electric force), ρ the charge carried by the ions in 1 cubic cm. of the air, F the vertical electric force, h height above the ground, and γ the vertical current per square centimetre, then, since

$$\gamma = k\rho F$$

and

$$\frac{dF}{dh} = 4\pi\rho = \frac{4\pi\gamma}{kF},$$

$$F_h^2 - F_1^2 = \frac{8\pi\gamma h}{k}$$

F_h and F_1 being the vertical electric force at a height h and at the surface of the ground respectively.

The vertical electric force F_1 immediately over grass-covered ground cannot much exceed 30,000 volts per metre ($=1$ unit of electric force in e.s. measure).

Let us assume that just below the cloud the electric force approaches the sparking value, 30,000 volts per centimetre, or 100 in e.s. measure. Then at all except quite small heights, F_1 will be negligible in comparison with F_h, and we have approximately

$$F_h^2 = \frac{8\pi\gamma h}{k} \text{ or } \gamma = \frac{F_h^2 k}{8\pi h}.$$

Assuming that the base of the cloud is at 2 km., and that the vertical force there almost reaches the sparking limit, and that the mobility of the ions is that of the ordinary small ion, we find for the magnitude of the vertical current about ·6 e.s.u. per square centimetre per second or 2 amperes per square kilometre. The number of ions per cubic centimetre (of mobility of the order of 1 cm. per second for a field of 1 volt per centimetre) would vary from something of the order of 3×10^6 near the ground, where the field is of the order of 300 volts per centimetre, to about 1/100 of this just below the cloud.

The current and ionization found above are those which would exist if the electric field below the thundercloud were maintained almost up to the sparking limit. The field immediately below the cloud could not, as a result of separation of charges within the cloud, reach the limit required for a discharge to earth, unless the rate of separation of charges exceeded the value found above for the current. Thunderstorms, in which discharges pass at frequent intervals between the base of the cloud and the ground, are, of course, quite common. It would, however, be unsafe to assume that the conditions in such storms are necessarily like those we have assumed. It is, for example, possible that the field below the cloud may only approach or reach the sparking value as a result of discharges of the upper pole of the nature of those discussed in section (I); the large currents and ionizations would in such cases only last for a very short time.

III. Production of Penetrating Radiation.

As I have elsewhere pointed out,* intense electric fields which, like those of thunderclouds, extend over considerable distances, may have important effects if β-rays are present. A β-particle is, under normal conditions, continually losing energy in ionizing and otherwise affecting the atoms which it traverses. The rate of loss of energy per centimetre of path is less the faster the particle is moving, varying approximately as the inverse square of the velocity. In air at normal pressure the mean rate of loss of energy per centimetre for a particle of which the kinetic energy is the equivalent of 20,000 volts (range about 1 cm., velocity about $\cdot 9 \times 10^9$ cm. per second), is about 10,000 volts per centimetre. A field of 10,000 volts per centimetre directed along the path of such a particle would, on the average, just compensate for this loss, and would more than compensate for the loss and give a balance of acceleration if its velocity were any greater. In a field of 20,000 volts per centimetre (only two-thirds of the sparking field) the energy gained by a 20,000 volt β-particle would exceed the loss by about 10,000 volts per centimetre. The faster the β-particle moves the smaller is the rate at which it loses energy in collisions; the rate of loss will approach a limit of less than 1,000 volts per centi-

* Camb. Phil. Soc., Nov. 24 (1924).

metre as the velocity becomes comparable with that of light. The chances of accidental deflections out of the accelerating field also become less and less as the energy of the particle increases.

There can be no doubt that β-particles are continually being emitted in the strongest parts of the field of a thundercloud (where the field approaches the sparking limit), as elsewhere in the atmosphere. A considerable proportion of these, and of others secondarily produced, must under the influence of the field acquire large additional energy. The greater the energy acquired by the particle the greater is its chance of surviving to gain more energy. The net rate of gain of energy by a particle which has travelled more than a few centimetres in the strongest part of the field may exceed 10^4 volts per centimetre, or 10^6 volts per metre. Thus, β-particles which have traversed a few metres in the direction of the field have already acquired energies exceeding those of the fastest known β-particles from radioactive substances. And the energy gained from the field will continue to exceed that lost in collisions even when the particles have reached regions where the field has fallen to 1,000 volts per centimetre.

A particle may thus acquire energy corresponding to the greater part of the whole potential difference between the poles of the thundercloud, which may be of the order of 10^9 volts. Such a particle may be expected to have important effects (when, for example, it strikes the nucleus of an atom), as its mass will be comparable with that of the hydrogen nucleus.

It has been assumed above that the air is at atmospheric pressure. The results remain essentially the same at low pressures; the maximum electric force which can exist in the air without discharge is proportional to the density, while the length of path required for a given energy loss by a particle of given velocity is inversely proportional to the density. For corresponding fields (which are proportional to density) the net gain of energy in corresponding distances (which are proportional to the density) remains the same; the linear dimensions of the β-ray system are inversely proportional to the density. At great heights where the electric force is much reduced the deflecting action of the magnetic force becomes important, and the β-rays will tend to run mainly along the direction of the magnetic lines of force.

The corpuscular radiation originating in the field of a thundercloud and the γ radiation which it may excite will constitute a penetrating radiation very similar in character to that which is known to occur normally in the atmosphere.

THE EVIDENCE OF TERRESTRIAL MAGNETISM FOR THE EXISTENCE OF HIGHLY IONIZED REGIONS IN THE UPPER ATMOSPHERE.

By Prof. Sydney Chapman, *M.A., D.Sc., F.R.S.*

Abstract.

Balfour Stewart's "atmospheric dynamo" theory of the daily magnetic variations is described; the earth's permanent magnetic field is the magnetic field of the dynamo; the atmosphere (undergoing small, but widespread, motions owing to thermal or tidal causes) is the moving armature; and particular ionized regions in the upper strata correspond to the "windings" in which the induced currents flow. The variations of ionization are the chief cause of the changes of intensity in the diurnal magnetic variations, as between day and night, winter and summer, and sunspot minimum and sunspot maximum. Hence these changes in the magnetic variations indicate the changes in the ionization of the atmospheric regions concerned. It is suggested that there are two modes of ionization, both due to the sun, and affecting different regions; one region, ionized by ultra-violet radiation, is a practically world-wide layer, possibly at about 50 kilometres above the surface; this is most intensely ionized directly under the sun in the sun-lit hemisphere, and the ionization diminishes during the night. The other region consists of the polar caps within the auroral zones, and especially these zones themselves; this region is ionized by the impact of corpuscles emitted from disturbed regions on the sun's surface.

I.

THE earth's magnetic field, and its variations, are known from observations in many parts of the earth; these observations are practically confined to the earth's surface. Very little of this field can be ascribed to currents flowing across the earth's surface or in the immediately adjacent atmosphere; hence the field has a potential in this region. By spherical harmonic analysis we can determine what proportion of the field originates within the earth, and what originates above the surface. The main "permanent" or secularly varying field is thus shown to be almost wholly of internal origin. The same method can be applied to the superposed fields which are responsible for the "rapid" magnetic variations, using the word rapid to denote the more or less regular *daily* variations, as well as the irregular changes known as magnetic disturbance (or, in extreme cases, magnetic storms). The analysis—first made by Schuster—shows that these rapidly varying fields are partly internal and partly external in origin, but that the external portion is from twice to thrice as great as the internal one. The internal part can be explained satisfactorily as a secondary effect of the external part, being produced by the currents induced within the earth by the varying outer field. The conductivity of the earth required to account for the numerical relationships between the two fields is of reasonable magnitude, and estimates derived from different components of the varying field are in good agreement. The theory of these variations is thus reduced to that of the external part of the fields.

II.

Many causes have been assigned for these varying external magnetic fields, and no theory, as yet, is without its difficulties. So much is required, for any theory

to be regarded as at all satisfactory or promising, that but few of those proposed have survived even a brief examination. The magnetic fields to be explained are specified by three variables (the north, west, and vertical components of magnetic force) at each place, and each of these variables is a function of latitude, longitude and time; any theory of their origin must co-ordinate these numerous facts, must indicate the situation, form and changes of the current-systems (lying somewhere above the earth's surface) which produce the fields, and must explain the causation of these currents. Many of the postulates contained in such a theory must, unfortunately, be incapable of direct confirmation by observation and measurement, whether the currents are supposed to flow outside the atmosphere or within it; for their intensity, as a simple calculation readily shows, is necessarily so considerable that even if they flow within the atmosphere it can only be in highly rarefied regions near or beyond the present limits of accessibility. None the less, the facts to be explained are so many and varied that any theory which by fairly simple hypotheses can satisfactorily account for all, or nearly all, is unlikely to be substantially wrong.

III.

Balfour Stewart gave the essence of what is now the generally accepted theory of the daily magnetic variations, and the subsequent great advances in our knowledge of physics in general, and atmospheric physics in particular, have steadily added support to what at the time was a bold piece of speculation. According to this theory, the currents which produce the daily varying magnetic field flow in the atmosphere, and therefore, considering the relatively small depth of the air, the direction is more or less parallel to the earth's surface; and they are induced by large-scale motions of the atmosphere in the presence of the earth's permanent magnetic field. The electromotive forces thus induced can take effect, however, only in some highly conducting region of the atmosphere.

IV.

On this view, three main factors determine the character and intensity of the electric currents and their associated magnetic fields. The first is the earth's permanent magnetic field, which may be considered definitely known in intensity and distribution throughout the atmosphere. It is, on the whole, a very simple field, almost the simplest possible over a spherical surface; but it does, of course, vary greatly in intensity and inclination from one pole to the other, and these variations must powerfully affect the currents arising from any given atmospheric circulation. They are in this way of great assistance in providing checks on the general soundness of the theory; the observed facts agree well with such inferences as can be drawn from these considerations.

V.

The second factor in this theory of the "atmospheric dynamo" is the large-scale motion of the atmosphere, which may be likened to the motion of the armature in the field of the permanent magnet provided by the solid earth. The third factor is the conductivity of the atmosphere. The ordinary winds in the atmosphere

must themselves produce quite appreciable electromotive forces, but the electrical conductivity near the ground is too small for these forces to produce appreciable electric currents. The layer where the necessary conductivity exists must be at a considerable height in the atmosphere. The motion of the atmosphere at great heights is not known so definitely as the earth's magnetic field at such levels, but we have, at any rate, some knowledge of the forces acting on the atmosphere, and by means of wind and barometer observations near the ground we can infer something as to the motions higher up. Moreover, we are concerned only with the largest features of the atmospheric motion, because relatively local circulations have a smaller magnetic effect than the world-wide movements.

VI.

The case in which we can be most sure of the atmospheric motions is that of the lunar daily magnetic variation. If the "atmospheric dynamo" theory is correct, and we rule out (as is easily justified) any direct magnetic effect of the moon of the required kind, the only way in which it seems possible for the moon to act is by producing a tidal circulation in the atmosphere. The character of such a circulation can be inferred by dynamical theory: the pressure is increased to a maximum at the two ends of the diameter of the earth which at any given instant passes through the moon, and at other points on the earth the flow of air is towards or away from the nearer of these two points; the variations of pressure and motion are similar over the two halves of the earth visible and invisible from the moon—i.e., they are semidiurnal in character. Moreover, as the moon varies in distance from the earth, the tide-producing force varies as the inverse cube of the distance, the whole variation being nearly 40 per cent.; the tidal variations of wind and pressure should show similar changes. By the analysis of barometric observations the existence of a circulation showing these characteristics has been established.

VII.

The character of the electric current system and magnetic field corresponding to such a tidal circulation can be determined if we assume that there is a thin spherical layer in the atmosphere, surrounding the earth, of uniform conductivity, in which the electromotive forces generated by the atmospheric dynamo can take effect. The assumption that the conductivity is uniform is incorrect, as will appear later, but the predicted type of lunar daily magnetic variation agrees surprisingly well with the observed type, derived from the mean of one or more month's data. In particular, the observed variation as so obtained is semidiurnal, each force component at each station having two equal daily maxima and two equal daily minima at intervals of a quarter of a day. Moreover, the magnetic variation increases from lunar apogee to lunar perigee, as the moon's distance varies from its maximum to its minimum, in a similar way to the tidal circulation itself. This is strong evidence of the general truth of the theory, and justifies the further step of calculating the conductivity of the atmospheric layer from the known data, viz., (i) the earth's permanent magnetic field, (ii) the tidal motion of the atmosphere, and (iii) the observed intensity of the lunar daily magnetic variation. The calculated value indicates that the layer must be about equal in conductivity to a layer of copper, at ordinary temperature, 1 metre thick.

VIII.

This result is confirmed, as regards its order of magnitude, by the solar diurnal magnetic variation. The sun produces in the earth's atmosphere a circulation similar in type to the tidal circulation due to the moon; in the case of the sun the circulation is partly due to thermal and partly to tidal causes. This circulation should, on the present theory, produce electric and magnetic effects similar to those caused indirectly by the moon; this is found to be the case. The solar diurnal magnetic variation is about ten times as intense as the lunar diurnal magnetic variation, agreeing fairly well with the fact that the solar diurnal atmospheric circulation is about fifteen times as rapid as the lunar tidal movements. If, therefore, the corresponding electric current systems flow in the same atmospheric layer, the estimates of the conductivity of this layer derived in the two cases will be nearly equal.

IX.

The solar diurnal magnetic variation, however, is not semidiurnal in type (that is, the same in any two hemispheres separated by any plane through the earth's axis), although the atmospheric circulation which produces it has this character, like the lunar tidal circulation. The daily magnetic variation field is more intense during the day than during the night hours, that is, over the sunlit than over the dark hemisphere. This is a direct indication that the conductivity and ionization of the atmospheric layer in which the induced currents flow is greater over the sunlit than over the dark hemisphere.

X.

The lunar diurnal magnetic variation shows the same intensification during the hours of sunlight in a still more striking way. It has been already stated that in the mean of one or more months this magnetic variation, like the tidal circulation which produces it, is purely semidiurnal. But at any particular epoch in the month this is no more true than it is of the solar diurnal magnetic variation: both are intensified during the hours of sunlight. These hours occur at different lunar times in the course of the lunar month, owing to the motion of the moon round the earth; each lunar hour occurs at all solar hours equally in the course of the month, and receives an equal share of sunlight and darkness. Hence in the mean of a month the lunar daily magnetic variation has a purely semidiurnal character, though at no individual epoch is it of this type. In the lunar daily magnetic variation the moon governs the atmospheric circulation and the sun the ionization of the layer in which the induced currents flow, and owing to the relative motion of the two bodies their two functions are more clearly separated than in the case of the solar diurnal variation, in which the sun governs both the circulation and the ionization of the atmosphere.

XI.

One important remark remains to be made about the first two of the three factors which determine the action of the atmospheric dynamo, viz., the permanent magnetic field and the atmospheric circulations. Neither of them is liable to any considerable change either irregularly from day to day or systematically from sunspot minimum to sunspot maximum. Consequently any considerable day-to-day

change in the solar or lunar daily magnetic variations, or any considerable systematic change in the course of the solar cycle, must be due to the variation of the third factor, viz., the ionization and conductivity of the layer or layers in which the induced currents flow. This conclusion is of great importance, because it indicates a powerful means—almost, if not quite, the only one at our disposal—of estimating the variations of ionization at high levels in the atmosphere. It leads also to the inference that in spite of their great similarity, the solar and lunar diurnal magnetic variations are produced (partly, at any rate) in different ionized regions of the atmosphere.

XII.

In the middle belt of the earth, extending on both sides of the equator* and including the major part of the earth's surface, the solar diurnal magnetic variation (which for brevity will hereafter be referred to as S) is very much the same on magnetically quiet (or undisturbed) days as on average days at the same season. There is a difference between the two, but it is only a slight one, and it is very nearly true to say that S is the same from day to day, independent of the magnetic activity (i.e., irregular magnetic disturbance), except when the latter becomes unusually great.

XIII.

The behaviour of the lunar diurnal magnetic variation (which will be denoted by L) is in this respect quite different from that of S; the amplitude of L undergoes large changes from day to day, in general increasing or decreasing in unison with the magnetic activity. At some stations (in the middle belt of the earth) and in some force components, the amplitude of L on the five most disturbed days per month is fourfold as great on the five quietest days per month. These changes are quite without a parallel in S in the middle belt of the earth, and if, as is here concluded, they are due to variations of ionization in the layer or region where L is generated, this region must be different from that in which S is produced.

XIV.

There is a further difference between S and L which indicates the same conclusion. While S does not vary from day to day at the same season, it varies considerably from year to year, its amplitude at sunspot maximum being 40 or 50 per cent. greater than at sunspot minimum, with but little change of type. The ionized layer in which S is produced must therefore vary in conductivity by this amount, and since the ionization is proportional to the square root of the intensity of the ionizing agent, the latter must be nearly twice as great at sunspot maximum as at sunspot minimum. On the other hand, it seems now to be definitely established that the average value of L varies only slightly from sunspot minimum to sunspot maximum, probably by less than 10 per cent.; the fact is the more remarkable because disturbed days, on which L (at any epoch) is increased, are undoubtedly more numerous at sunspot maximum than at minimum, and this in itself seems sufficient to account for the small observed increase of L at maximum epoch.

* So far north, for example, as Greenwich or Kew.

XV.

Beyond the middle belt of the earth, in the polar regions, irregular magnetic disturbance or activity is much more intense than in lower latitudes, though its proportionate changes of intensity are, on the whole, common to the earth as a whole. Magnetic disturbance has its origin in the polar regions, and in the middle belt of the earth it is seen only in an attenuated form. Intimately associated with the irregular disturbance is the special form of the solar diurnal magnetic variation observed in the polar regions ; this, like L, varies greatly from day to day in rough proportion to the degree of disturbance, and it is, in fact, a much attenuated extension of this portion of S into the middle belt of the earth that produces the slight differences, already mentioned, shown by S in this region as between quiet, average and disturbed days at the same season and solar epoch.

XVI.

A detailed study of the daily magnetic variations in the polar regions convinces me that over each polar cap there is in the upper atmosphere a roughly circular electric current circuit, which seems usually to correspond approximately in position with the zone of maximum auroral frequency. The latter zone is nearly circular, with its centre near the pole of the axis of magnetization of the earth, and with an angular radius of about 23°. Since auroral and magnetic disturbance are very closely associated, the two zones are probably identical, in which case the current must flow in the belt ionized by the electric particles from the sun which give rise to the aurora. This implies that the current is upwards of 90 kilometres above the surface of the earth. The current appears to flow from east to west, but to be of unequal intensity round the zone, some current finding its return flow over the polar cap from one point of the belt to the other, nearly parallel to the meridian plane through the sun. Thus, while the zone is specially conducting, the whole polar cap within the zone would also appear to be ionized at a similar level but to a lower degree.

XVII.

The conception which I have formed of the larger features of the electromagnetic phenomena in the polar regions and elsewhere in the upper atmosphere is different in some important respects from one which I described a few years ago in a Paper entitled "An Outline of a Theory of Magnetic Storms." It can only be sketched very briefly here, prior to the publication of several investigations, partly completed and partly in progress, giving in detail the data and the arguments in support of these conclusions. At the basis of the theory is the hypothesis that in the polar regions there is an atmospheric zone or ring of very variable and intense conductivity, associated with auroræ. A poleward motion of the atmosphere at auroral heights in this latitude would supply electromotive forces of the right type to account for the westerly current along the zone which has already been mentioned. Very little direct evidence as to the motion of the atmosphere at auroral heights is available, but a provisional estimate of the velocity of the poleward motion proves to be of not excessive magnitude. The lunar tidal circulation must also extend to auroral heights, and it seems probable that its current production reaches its maximum in the auroral belt, owing to the high average value of the conductivity there.

XVIII.

The currents produced in the polar regions by these two sets of atmospheric motions must be accompanied by relatively intense magnetic fields in the neighbourhood of the zones, but in the middle belt of the earth these fields are almost inappreciable. The disturbance field and the lunar diurnal variation field observed in the middle belt of the earth appear to be the fields of induced currents, flowing partly in the earth and partly in the atmospheric ionized layer, extending over the greater part of the earth, in which S is produced. That varying currents in the polar regions could by induction in the atmosphere and the earth produce currents which would have considerable magnetic effects in the middle belt of the earth is not by any means obvious, but it is a conclusion indicated by calculations, necessarily of a rather complicated nature, in which Mr. T. T. Whitehead has co-operated with me.

XIX.

As regards L, this theory is a modification of the simple theory previously explained. The actions there described must occur, but it would seem that the L-field so produced is a minor part of the whole, and that the major part of the current system associated with L which flows in the ionized layer over the middle belt of the earth is a secondary system induced by primary currents flowing in the independent highly-ionized polar regions. Many questions raised by this revision of the theory of the lunar diurnal magnetic variation are at present under examination.

XX.

The conclusions relative to the ionization of the upper atmosphere, which can be drawn from the evidence (here briefly and inadequately described) of terrestrial magnetism, may be summed up as follows: There are two independent regions of high conductivity, ionized by independent solar agencies. One of these regions is a layer extending nearly or quite over the whole earth; its conductivity is greatest where the sun's zenith distance is least, and therefore it varies throughout the day and night at any one station; when the station is in the sunlit hemisphere the conductivity is much greater than during the night hours. The average conductivity of this layer is comparable with that of a layer of copper, under ordinary conditions, one metre thick. The conductivity increases considerably (by 40 to 50 per cent.) from sunspot minimum to sunspot maximum. The distribution of conductivity in this layer suggests that the ionizing agent is ultra-violet radiation; it seems likely that the ionization is associated with the production of the layer of ozone which is known from the work of Fowler and Rayleigh to exist in the upper atmosphere. Consideration of the absorption of the ozone bands of ultra-violet radiation indicates a height of about 40 or 50 kilometres for this layer; the *magnetic* evidence does not suffice, so far as the theory is developed at present, to indicate the height of the layer.

XXI.

The other ionized regions are the auroral zones round each pole, and the polar caps within these zones. The polar situation of the zones is satisfactorily explained by the theory of Birkeland and Störmer, viz., that it is due to the deflection in the

earth's magnetic field of streams of charged particles coming from the sun, which must necessarily enter the atmosphere in a fairly limited region round each pole. Measurements of the height of auroræ indicate that these particles penetrate the atmosphere down to about 90 kilometres above the ground ; the auroræ are observed up to a height of several hundred kilometres, so that the zone of ionization must be very deep, though the greatest conductivity probably occurs near the lower limit. It should be possible, by means of suitable magnetic data, to determine the average total conductivity of this zone, but at present this has not been done.

XXII.

There is good evidence that the streams of corpuscles which ionize the auroral zones and polar caps are associated with disturbed regions on the sun, and that the irregular occurrence of magnetic disturbance is due not only to the variability of these solar regions, but also to the rotation of the sun, which constantly changes the direction of emission of the streams. The ultra-violet radiation which ionizes the world-wide atmospheric layer probably proceeds from the solar surface as a whole, and its variation throughout the solar cycle indicates that the sun's surface in general, and not only the specially active parts of it, undergoes important changes in the course of the solar cycle.

GENERAL DISCUSSION.

Dr. G. C. SIMPSON maintained, with regard to Mr. Wilson's contribution, that no experimental or observational evidence has yet been produced to show that thunderstorms ever have negative electricity under positive electricity, and until this is done he is unable to accept Mr. Wilson's explanation of the origin of the normal negative charge on the earth's surface. Prof. Chapman's theory is open to the objection that the air-tides produce only very small differences of pressure even at the surface of the earth, while at the height of the conducting layer which he postulates (50 km.) such differences would be minute. They could hardly, therefore, influence the motion of the atmosphere to the extent demanded by his view of the matter. In connexion with Prof. Appleton's Paper, it was to be noted that the strength of signals received from a sea station had been observed at Slough and at Orfordness, the latter station lying about half-way between the other two. During the daytime there was no substantial error in the direction found by either of the land stations. At night there was no error at Orfordness, where the signal was received purely over the sea ; but at Slough a large variation in intensity and apparent direction was introduced by the land passage. It seems incredible that for three stations so close together such a marked difference could be caused by a conducting layer 60 km. up, and the latter also failed to account for the effect of the change from daylight to dark. Mr. Watt had shown that atmospherics may originate where there is no thunderstorm, but an atmospheric demands an electric discharge of some sort. What can be the nature of such a discharge in a cloud which is not a thunder-cloud ?

Prof. C. L. FORTESCUE : The opening Paper by Dr. Eccles and the subsequent Paper from Prof. Appleton have epitomised the existing knowledge with regard to the Heaviside layer and the part which it plays in the transmission of wireless signals. Neither of these two speakers, however, has dealt with the question of the sudden transition from high conductivity to low conductivity which appears to be required if this layer is to play the part assigned to it. Dr. Wilson's contribution suggested a possible explanation of a sudden change in the conditions of the upper atmosphere. He pointed out that above the charged cloud the electric field distribution was one which made it possible for ionization to take place in the upper air, and he suggested that this ionization might be either of a stable or unstable type. In the former case it would appear that the change from the ionized state to the unionized state would be sharply defined, and the conditions would be those necessary for the Heaviside layer as assumed. This possible explanation, of course, assumes that there are charged zones in the lower atmosphere. In view of the observed high voltage gradients at all times it seems probable that either these charged layers are always present, quite irrespective of the phenomena of thunder and lightning, or that corresponding electric fields are set up in some other way.

Mr. Watson Watt's Paper is of great interest to wireless engineers who have endeavoured to find an explanation of the devastating effects of atmospherics in practice. In his earlier Papers he describes wave forms in which the time interval was several milli-seconds. In this present Paper he has shown wave forms of time interval not exceeding 25 micro-seconds, and many of these wave forms have a complex form. Moreover, he mentions the fact that extremely short period disturbances have been observed which are of large amplitude and of such short time interval that their form cannot be determined. When it is remembered that the cathode ray oscillograph does not respond accurately for frequencies much in excess of 50,000 it will be seen that later information points to the existence of violent atmospheric disturbances of extremely short duration. These disturbances would undoubtedly account for the observed effects in wireless receiving circuits, and the fact that 87 per cent. of these atmospherics have been traced to meteorological disturbances obviates the necessity for searching for some source of interference outside the atmosphere.

Dr. J. ROBINSON said that very little had been heard that evening about the effect of the earth's conductivity, yet the importance of this was shown, for instance, by the experiment of Dr. Smith-Rose quoted by Dr. Simpson on the passage of wireless signals over land and over sea. A type of propagation formula different from those hitherto considered would apply to signals sent from one aeroplane to another. It is known that transmission between 'planes is better than transmission between ground stations, and, further, that with a wave-length greater than the natural wave-length, end-on propagation occurs. Facts like these appear to demand experimental investigation.

Mr. C. E. P. BROOKS (communicated) : The distribution of thunderstorm frequency has

recently been investigated in the Meteorological Office and a Geophysical Memoir embodying the results is now in the press. It was found that the average number of thunderstorms a year per station over the whole world was sixteen. It was assumed that each station will record those thunderstorms which develop in or enter a circle of area 200 square miles surrounding the station, so that the earth experiences about sixteen million thunderstorms a year, or 44,000 a day. Estimating the average duration of a thunderstorm as an hour, this means that there will be in progress at any one moment about 1,800 thunderstorms in different parts of the world, The frequency of lightning flashes during a storm was taken as 200 per hour, giving a total of 360,000 flashes an hour, or an average of 100 per second.

Within a circular region with a radius of 1,000 km., centred at London, the average frequency of thunderstorms is about 11,500 during the six months October to March, and 56,500 during the six months April to June. Presumably, the average storm in these latitudes is less severe than the average tropical storm ; assuming a frequency of 25 lightning flashes per storm in winter and 50 in summer, the number of flashes is about one a minute in winter and eleven a minute in summer. The winter flashes are almost independent of the time of day, the summer flashes are most frequent in the afternoon.

Mr. Watson Watt's charts showing the seasonal distribution of apparent sources of atmospherics fit in very well with the seasonal variation of thunderstorm frequency. In winter thunderstorms are relatively more frequent near the Atlantic coast of Europe, and the occasional storms of Iceland and the Faroes are practically limited to this season. Winter thunderstorms are rare in the interior of Europe. In summer the frequency is greatest in the interior, and thunderstorms are rare on the Atlantic coasts of the British Isles. There is an important maximum of thunderstorm activity on the northern side of the Alps, which appears to be indicated on the summer and autumn charts of atmospherics.

Mr. F. E. Smith, C.B.E., F.R.S. (Chairman), expressed the thanks of the meeting to those who had taken part in the discussion, and also the hope that the two Societies might hold a further joint meeting in the future.

REPLIES TO THE GENERAL DISCUSSION.

Dr. W. H. Eccles, in closing the discussion at the Meeting, said :—

The discussion has shown that the subject of atmospheric ionization is so very wide that there is difficulty in grasping all the facts and seeing their connexion with one another.

The broad questions left unanswered are : How many conducting layers are there in the atmosphere ? What is the position of each of these layers ? Which of them function in the propagation of wireless waves ? In which of them reside the sources of the strays or sturbs that trouble the wireless telegraphist ?

So far as radio telegraphy is concerned, Prof. Appleton and Dr. Simpson have cited observations which prove the great influence of land and sea and of the air above them. That the air has an influence might be expected from the data of atmospheric conductivity discussed by Dr. Chree. It is proved also by the observed connexion between weather and signals. For instance, tropical storms cause variations of signal strength ; the variations resemble those produced by solar eclipses, in which the darkening of the air improves transmission. It seems that the troposphere is the region of the atmosphere concerned in these phenomena, but perhaps the turmoil due to a cyclone reaches into the stratosphere. The stratosphere, or, as it is sometimes called, the isothermal layer, has, rather unexpectedly, escaped attention, unless Prof. Chapman's estimate of the height of the seat of the magnetic variations is not beyond the isothermal layer. Here the question arises : Is the terrestrial magnetician's ionized layer the same as the wireless man's ? We are still undecided. As a fact, the wireless man and the magnetician each want at least two conducting layers. Wireless phenomena can scarcely be understood at all without the aid of, firstly, a world-wide permanently ionized layer, and secondly a lower layer subject to daily ionization ; magnetic variations cannot be explained without the aid of a world-wide layer ionized daily together with a layer permanently ionized and mainly confined to the polar regions. The disparity between our requirements is at first sight regrettable, but it will doubtless lead to interesting hypotheses and discoveries.

The discussion did not bring out any information about the effects of auroral displays and magnetic storms on wireless propagation, so I might recall that the British Association Committee analysed ten years ago a great many far Northern observations, and could not find any real connexion between auroræ and wireless transmission. Moreover, no connexion has anywhere been observed between magnetic storms and wireless. This seems to indicate that wireless waves do not penetrate into the auroral layer. Perhaps the bottom of this layer, at a height of about 100 kilometres, is the impenetrable quasi-reflecting ceiling named after Heaviside.

Time does not permit me to summarise the mass of valuable work performed by Mr. Watson Watt on atmospherics (some of it in collaboration with Prof. Appleton). But we must thank Mr. C. T. R. Wilson for his illuminating suggestion that thunderclouds may by their electrostatic field ionize the rarefied air beneath a high conducting layer. This may help to elucidate the fact that strays or sturbs are often produced without visible lightning, and that a thunderstorm between two stations frequently affects inter-communication.

Prof. E. V. Appleton (communicated) : In reply to Prof. Fortescue, I do not think it is necessary to assume a reflecting layer with a very sharp boundary for night-time conditions. For mathematical purposes it has had to be assumed that the Heaviside layer acts as a spherical shell with a sharp boundary and a conductivity of the order of that of sea-water. But for such a boundary long waves would be reflected better than short waves, and we know that the reverse is the truth. The correct explanation is, I think, the one given in the Paper. Ionic refraction takes place in a region in which the ionization increases with the height and in which the pressure is so low that the time between the collisions of an electron and the gas molecules is longer than the period of the wave. The lower atmosphere is devoid of excessive ionization due to the rapid recombination of the ions after the geometrical sunset.

Prof. Fortescue has discussed the experiments of Mr. Watson Watt and myself on atmospheric wave-forms from the standpoint of the wireless engineer, and suggests that there may be atmospheric disturbances of so short a period that the oscillograph will not respond accurately to them. I take it that he comes to this conclusion because atmospherics are still troublesome, at times, on wireless circuits of time period of the order of 1 micro-second. But I think it will be found that large pulses of duration of 25 micro-seconds are capable of producing all the effects on short-wave receivers with which we are familiar. The notion that the pulse must be of the same order of duration as the time period of the circuit before the disturbance is serious seems to have originated in an article by Mr. T. L. Eckersley, who considered mathematically the effect of a

pulse on a wireless receiver. Eckersley took E_0a^2/ a^2+t^2 as the expression for the pulse, E being the electric force and t the time, and showed that the effect of such a pulse on a wireless receiver was negligible, except when the time period T of the receiver was $2\pi a$, i.e., of the same order as the duration of the pulse. But I do not think that the use of such a type of pulse is justified. Let us consider for a moment the analogy between the atmospheric problem and the white light problem of physical optics. We may consider the energy of a pulse as distributed in its energy-spectrum. If we proceed in this way it may be shown that various pulses differ enormously in their energy-spectra. For example, in the type of pulse considered by Mr. Eckersley the energy is confined to a narrow band of the range of commercial wavelengths, so that the result he got is not surprising. On the other hand, if we use $E_0e^{-\lambda t} \sin \lambda t$ as the expression for the pulse, we get a spectrum with energy distributed through a large range of wavelengths. It is interesting that this latter expression, which represents a very strongly damped discharge, gives an energy-spectrum closely resembling that deduced from the effects of atmospherics on systems of different wavelength, while the expression used by Eckersley gives no agreement. We therefore get further support for our belief that atmospherics originate in strongly damped discharges.

Thunderstorms have often been considered as a negligible source of atmospherics, but I have always maintained that lightning flashes and discharges from thunderclouds could produce all the effects observed. A careful watch should be kept for further evidence of the origin of atmospherics in shower clouds when the latter can be seen at the receiving station. My own observations on near thunderclouds and shower-clouds have been that there have been no atmospherics, except with the flashes. But many observations of this kind would have to be made before we could pit this against Mr. Watson Watt's conclusion that only a quarter of the atmospherics are due to thunderstorms. There seems little doubt that the thunderstorm mechanism is responsible for the separation of charges, which brings about the discharge.

Like Dr. Simpson, I cannot accept the mere correlation of atmospherics and rain. The radiation field of the atmospheric indicates a discharge, and one of no small magnitude.

With reference to the thundercloud origin of atmospherics, and Mr. Brooks's most valuable information on thunderstorm frequency, I may perhaps be permitted to quote my remarks of a year ago from the Proceedings of the Institution of Electrical Engineers (6th February, 1924).

" Some years ago (see " Year-book of Wireless Telegraphy and Telephony," 1921, p. 1114) I pointed out that Mr. C. T. R. Wilson's observations on the electric field of thunderclouds might be used in deducing the order of magnitude of the electromagnetic disturbance arising from lightning flashes. From these measurements we may deduce that a lightning flash is an aerial with an effective height only to be measured in kilometres and with a rapidly changing current of the order of 20,000 amperes. There seems no doubt that thundercloud discharges can be of sufficient magnitude to account for the radiation fields observed. It has often been argued that it is difficult to imagine sufficient lightning flashes on the earth's surface to account for the number of atmospherics observed, but I do not think that this is the case, as the following considerations show. It is known that the earth as a whole remains negatively charged in spite of the positive atmospheric current flowing into it, which is of sufficient magnitude to neutralize the charge in 10 minutes. The chief problem of atmospheric electricity is to determine how this negative charge is maintained. Mr. Wilson's theory is that the negative charge is replenished by the electricity from thunderclouds either in the form of lightning flashes or intense ionization currents. According to one version of this theory, we must regard the thunderstorm regions of the earth's surface as sending positive charges to the Heaviside layer and negative to the ground, either in the form of lightning flashes or ionization currents, and thus maintaining the Heaviside layer at a million volts positive with respect to the earth. In accounting for the maintenance of the earth's charge in this way it may be estimated that at least 1,000 thunderclouds are in action at any one time. I have tried to link up this theory of the thunderstorm origin of atmospherics with the directional observations of Hoyt-Taylor, Round, Watson Watt, and Schindelhauer on atmospherics, and it appears possible to get satisfactory correlation if we assume that the thundery regions are situated mainly on a wide equatorial belt (and in particular South Africa and South America), which acts as a permanent source of atmospherics. If we accept the theory the next point is to consider what general rules it gives as to the average direction of arrival of atmospherics at any point on the earth's surface, and what form the diurnal variation will take. It is a well-known fact that there is a correlation between local time and thunderstorm frequency. Thunderstorms occur most frequently during the afternoon, and thus at that time we may expect places having the same longitude as the point in question to act as sources of atmospherics. So far as this point is concerned, the source of atmospherics should

follow the sun. Secondly, there is the question of the proximity of a particularly thundery district either on or off the equatorial belt (e.g., a land area as opposed to a sea area). This region will be a marked source of atmospherics when its local time is afternoon. And, thirdly, we have to consider the question of the ease of transmission of electric waves through the atmosphere between the point of origin and the point of observation, and the diurnal variation of the transmission factor. According to Mr. Wilson's theory, the earth should possess its maximum negative charge when the maximum number of thunderstorms are in action at any part of the day. Now Mauchly has shown that the charge on the earth reaches a maximum about 18 G.M.T., and if we examine Schindelhauer's results we find that the atmospheric disturbances coming from the south and south-west reach a maximum at about the same time. This points to the conclusion that South Africa and South America may be regarded as the centres of gravity of the earth's thunderstorm and atmospheric-producing regions."

Dr. CHREE (communicated) : As there has been a good deal of indefinite talk about conducting layers in the atmosphere, I would offer a few remarks on the subject. If, as seems reasonable, aurora is accepted as a direct evidence of high conductivity, then according to Störmer's observations, the "layer" of high conductivity extends at least sometimes to above the 600 km. level. The lower edge of some types of aurora is very clearly defined, and in such cases Störmer's measurements indicate from 90 to 110 km. as the lowest level. There may at times be aurora possibly of a more nebulous type at lower levels, but the more recent Norwegian height observations afford no confirmation of this. According to Störmer's observations, what we may look for during aurora is high conductivity from a height of several hundred kilometres, possibly from the top of the atmosphere down to about 100 km. The conducting layer during aurora may have a sharply defined lower edge, but it is certainly not a thin layer.

On the side of terrestrial magnetism, the association of magnetic disturbance with brilliant changes of aurora, the 11-year period in the amplitude of the regular diurnal variation, and the 27-day interval in magnetic storms, all point to the upper atmosphere as the locus of the electric currents to which the regular and irregular diurnal variations of the magnetic elements are ultimately due. But I do not think that any final conclusion can yet be drawn from the magnetic phenomena as to the exact height of the conducting layer or layers, or as to the existence of more than one layer.

Prof. S. CHAPMAN (communicated) : With regard to Dr. Simpson's objection concerning the smallness of the pressure-differences associated with the lunar atmospheric tide, it must be remembered that these pressure-differences are not the cause of the tide, but are themselves, like the tidal motion, consequences of the gravitational tide-producing force. The latter is proportional to the mass of the element of air acted upon, and therefore tends to produce the same tidal acceleration of the air at high levels as at low levels.

I agree with Dr. Chree in doubting the appropriateness of the word "layer" for the auroral region of high conductivity, which may (in its most intense part) resemble a nearly vertical sheet rather than a flat layer.

Mr. R. A. WATSON WATT (communicated) : Prof. Fortescue's wide interpretation of my data as showing that we need not go outside the atmosphere in our search for the source of atmospherics is hardly justified by the data, which refer specifically to the selected cases in which short-wave direction-finding stations were able to observe a well-marked direction of arrival of atmospherics. Even so, only half the total number of cases where simultaneous bearings were obtained yielded locations within the wide area examined. I think Prof. Fortescue's view is correct, but it does not follow from the data under reference.

It is very satisfactory to have, from one with Mr. Brooks's encyclopædic knowledge of available data, a considered estimate of the world's supply of lightning, and those concerned with the study of atmospherics will await with interest the publication of the full memoir. A large mass of data on the distribution of atmospherics in time and in direction of arrival awaits correlation with such thunderstorm data.

I am glad to have Mr. Brooks's confirmation of my reading of the seasonal distribution charts, which I regarded as in close agreement with thunderstorm data.

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